THURSDAY, JANUARY 5, 1899.

AN EVOLUTIONAL POLEMIC.

Organic Evolution Cross-examined; or, Some Suggestions on the Great Secret of Biology. By the Duke of Argyll, K.G., &c. Pp. vi + 201. (London: John Murray, 1898.)

IT has always appeared a mystery to the writer of this notice why the phenomena of life should be dealt with by some men of science and by certain philosophical writers in a totally different spirit to that in which other groups of natural phenomena are considered and discussed. It is true that we know less about life than about other phenomena—it is true that the organic world is full of unexplained mysteries. Equally certain is it that the living organism can accomplish physical and chemical feats by processes which we are now ignorant of, and which we cannot at present imitate. But it is not obvious why because a particular department of knowledge, by virtue of its inherent difficulties and intricacies, happens to be in a different phase of development from other branches of human inquiry, that the whole domain of organic nature should be detached and delimited, and put on quite a different plane to any other department of science.

Anthropologists might offer a reasonable explanation of this difference of attitude by an appeal to the history of the development of natural knowledge. The early observers of nature and the writers of the ancient cosmogonies were not impressed by the slow and orderly course of the inorganic world in the same way that they must have been familiar with their organic environment. The facts and laws of physical science required something beyond mere casual observation for their elucidation, and the manifestations of these laws became impressive only when they reached the dignity of cataclysms. On the other hand, these writers were constantly being brought into contact with the living world in a hundred ways that had a more or less direct influence on their everyday lives. They must have noticed the plants and animals of the districts which they inhabited; the individuals of their own and other races must have been of more direct importance to them than the unobtrusive sequence of non-vital phenomena. It is not to be wondered at that in the ancient cosmogonies the living world should have been regarded in a different light to the world of "dead" matter, and a special mode of origination invoked. In brief, there has arisen a set of ideas which are even broader than "anthropocentric," and which might fairly be designated *biocentric*, and it is these ideas which, consciously or unconsciously, permeate the work now under consideration and all similar productions.

The Duke of Argyll will no doubt disclaim any such severance of vital and non-vital phenomena. In certain passages he states somewhat explicitly that he wishes it to be understood that he deals with nature as a whole in this cross-examination of evolution. But it will be evident to those who have followed the course of thought in this field, that this latest contribution from the doughty opponent of Darwin and Spencer and Huxley and Wallace is nothing but a compromise between the ancient biocentric system and the newer ideas of the order and sequence of nature. It is a kind of eviscerated Bridgwater Treatise with an aggressive binding, and with the honest and plain teleology of the writers of those famous old volumes replaced by the word Plan with a capital P. If we are not mistaken, there was a period in the author's literary career when he scouted the idea of evolution in any form. Be this as it may, he now tells us that he accepts "the leading idea of development" (p. 98), and he even goes so far as to say that he holds this idea "to be indisputably applicable to everything, and especially to organic life." The same statement is repeated in other forms elsewhere in the book (p. 189, for example). The Duke apparently prefers the word "development" to evolution. There is a subtle distinction here which the ordinary reader might overlook, and which it is therefore desirable to point out. Evolution has become associated with development through external causes controlled by, and in co-operation with, causes resident within the organism. Such, at least, is the idea which the writer has always associated with organic evolution. Development on the other hand, is associated with a process of spontaneous growth by virtue of an internal agency only. This appears to be the burden of the Duke of Argyll's tale as told in the three essays composing the present work, which essays have been reprinted, with slight alterations, from the *Nineteenth Century*.

This notion of an internal force of development implanted in the organism by an external agency is a very venerable dummy. The Duke has tried to furbish it up with a fresh coating of paint, but evolutionists will, I am afraid, not consider the new garniture sufficiently attractive to claim their attention. The old figure is still there, and the dents made by the sticks thrown at it by such skilled marksmen as Huxley and Weismann are too deep to be effaced. We find, for example, on page 155, that an essential feature of the creed of the "mechanical evolutionists" (as interpreted by the author) is "the internal directing agency or force, which always pursues a definite line of growth, so that all the demands of the completed structure must have been present from the beginning, &c." This is considered by the Duke to be a necessary consequence of the belief of the evolutionist that the development of the germ is to be explained by "processes of ordinary generation." Why, it may be fairly asked, are biologists to be so constantly reminded in wordy essays that the characters and attributes and properties of organisms must have been potentially present from the beginning of life? The whole case of the biocentric school amounts to this, and nothing more. So all the characters of a complex mineral must have been potentially present in the material atoms of which it is composed; and if there is an internal directing agency in the case of a "procreated" germ, there is just as much an internal directing agency in the mineral compelling a definite crystalline structure and chemical composition. We have never heard of any essayist taking the writer of a mineralogical treatise to task because he had failed to indicate to his readers that the structure and composition of minerals were to be explained by innate properties conferred in accordance with a prearranged plan.

We have dealt so far with these essays in their constructive aspect, but they have also a destructive side;

and here we must in fairness to the author point out that he makes some good points out of Mr. Spencer's change of view with respect to the efficiency of natural selection. The whole of the first essay is in fact a kind of merry-making over Mr. Spencer's abandonment of that excellent child of his own creation, the term "survival of the fittest." We can safely leave the Duke in the hands of the veteran author of the "Synthetic Philosophy," but in so doing it may be well to indicate that many—perhaps we may say the majority of biologists in this country—have long ago parted company from Mr. Spencer on this question of the enhanced importance of "direct equilibration," and the subordinate position assigned to "indirect equilibration" in his later writings. When, therefore, evolutionists are withered with the reproach of being "mechanical" by the noble author of these three essays, *nous autres* can take comfort from the thought that it is those who in America are called the "Neo-Lamarckians," who are expected to realise the grossness of their conceptions.

The results of attempting to recast the old idea of "creation" in the mould of the modern theory of evolution are just those results to which all attempts at reconciliation appear to lead. What these results are can only be briefly indicated here; but if, as a study of mental attitude, the philosophical student will take the trouble to compare the destructive with the constructive side of the essays, he may find much material for his instruction. For surely it is instructive to find a writer using weapons for the demolition of an antagonist without apparently being aware that these same weapons are equally destructive when applied to his own position. The Duke is acutely critical in the first essay about Mr. Spencer's phraseology. He quotes with approbation Mr. Darwin's views about explanations which are good for everything in general, being good for nothing in particular (pp. 58-60). Every man of science will join hands with the Duke on this point. But after having indulged in such exceedingly great rejoicing over the abandonment of the hateful expression, "survival of the fittest," and all that is implied thereby, the author, in a later essay, lets us into the secret of his own view of the developmental process. It is all contained in the internal directive agency; it is—

"the kind of causation which is conspicuous in the preconceived Plan, in the corresponding initial structure, and in the directed development of vital organs as apparatuses prepared beforehand for definite functions" (pp. 192-193).

Now, as far as natural and physical science has any voice in this matter, it may be equally well said that everything that happens in the universe is in accordance with a preconceived plan. But why offer this as an explanation especially to be invoked in the case of vital phenomena? It must be equally true of gravitation which causes an avalanche to overwhelm a village, or of an earthquake or volcanic eruption which destroys a city. It is precisely of that order of "explanation" which is good for everything in general, and therefore for nothing in particular. In other words, it may be the statement of a general truth or it may not—the point is one that is outside the scope of scientific inquiry—but it explains nothing, and it leaves us precisely where we were before. Curiously enough, the author tries to make Huxley responsible for this kind of explanation with respect to the vertebrate skeleton—

"a Plan, laid down from its beginning, in its originating germs, with a prevision of all its complexities of adaptability to immense varieties of use. There must have been a prevision for these uses in certain elements and rudiments of structure, and in certain inherent tendencies of growth which were to commence, from time to time, the new and specially adapted structures" (pp. 161-162; also p. 120).

This is surely doing violence to Huxley's teaching; we can call to mind no passage in his works which bears this interpretation. We ask the Duke in fairness to Huxley to reperuse the fifth chapter of the second volume of Darwin's "Life and Letters."

The importation of ultra-scientific notions into the doctrine of evolution leads the author into all those other quagmires in which others have floundered before him. The summary of the Darwinian hypothesis, on pp. 60-61, is a travesty; the conception of variability, on pp. 108-109, is a totally inadequate statement of the actual state of knowledge; the reiteration of the epithets "mindless," "fortuitous," "haphazard," &c., as applied to variation, is an impeachment of Darwin's views which has been made over and over again, and which has been met over and over again. The attempt to hurry up the course of evolution, in order to meet the limits of time imposed by certain arguments from the physical side, leads the author to accept "discontinuous variation" or development *per saltum* (pp. 122-125). It may be of comfort to the Duke to know that Mr. Francis Galton will go some way with him here. But the analogy between the rapidity of individual development in some cases, such as in metamorphosis, and the rapidity of organic evolution, which is put forward as an original idea (pp. 120-124), appears to the writer to be a false analogy. The Duke's idea of discontinuous variation is given (p. 148) in the following words:—

"It is conceivable that species might be really as constant and invariable as we actually find them to be, for some long periods of time—embracing perhaps centuries or even milleniums—and then suddenly, all at once, evolve a new form which should be equally constant, for another definite time to follow."

This may be conceivable, but we should like to have some evidence of its probability. It involves not only a sudden departure or "sport" on the part of the individual offspring, but the simultaneous and similar aberration of all the offspring of a particular generation. Even the much-abused "mechanical evolutionist" has never made such a draft as this upon the resources of the speculative faculty. The old "internal developmental force" was in the minds of its supporters a respectable kind of agency that might be expected to come into operation when the exigencies of external conditions required it. But here we have a suggested mechanism of development which makes one shudder to think what might happen if there were the very slightest hitch in the adjustment between the characters of the new form, which appeared when the proper moment had arrived, and the external conditions under which the alarum, as it were, went off.

The discussion of rudimentary organs from the author's point of view (p. 162 *et seq.*) is one of the best illustrations of the effect of introducing ultra-scientific "explanations" into the domain of natural science that will be found throughout the book. These organs are not vestigial, but prophetic. Such a rudiment

"may be where it is—simply because it indicates an original direction of growth, or of development, which was made part of the vertebrate Plan from the beginning of the series, for the very reason of its potential adaptability to an immense variety of purposes. Moreover, the arrest of such tendencies of growth, at a given point in the series, may well have been part of the same Plan from the beginning" (pp. 175-176).

The general public, who have hitherto been accustomed to consider the essence of the theory of evolution to be contained in the statement that man is descended from a tailed ancestor, will no doubt hail this statement of the Duke's with acclamation, because the rudimentary tail (which is discussed on p. 157) may after all be only "an arrested tendency of growth." The other logical alternative, that it is a preparation for a tail to be developed by generations yet unborn, is quite legitimate from this point of view, but the author will doubtless not insist upon this deduction. Similarly the rudimentary teeth and pelvic limbs of whales, which are also somewhat fully considered, are not necessarily vestigial in the sense of being the remnants of structures that were at one time functional. These rudiments are likewise to be regarded as "arrested tendencies of growth," or else as predetermined preparations for the transformation of whales into land animals. Honestly we prefer the Darwinian explanation—even though we have to face the difficulty of the electric organs of the Torpedo.

We have discussed these essays in the spirit of controversy because they are purely controversial in character. They have been considered also at greater length than their scientific importance warrants. Strictly speaking, it is difficult to see what class of readers are influenced by writings of this kind; certainly not working biologists of any school of thought. But the unlimited hospitality extended by the editors of high-class popular magazines to essayists of the calibre of the author of the present work, shows that such writers have the public ear, and it seems desirable to let the public know that the authority which is wielded by these knights of the pen is not conferred by those whose special studies in the field of biology might be reasonably supposed to give them the right of conferring such authority. There are some minds that may be satisfied with the word "Plan" as an all-sufficient explanation of natural mysteries yet unsolved. Like "the blessed word Mesopotamia," it may convey much comfort to such minds; but the earnest seeker after scientific truth will not be deluded, and he will still go on groping his way towards a knowledge of the processes by which the universe has been evolved by the only legitimate methods of observation, experiment, induction and deduction. It may be that, as Darwin long ago suggested, a definite set of characters undergoing selection may by inheritance tend to go on varying in the same direction, and so give to the course of development an impetus as though from some internal agency. But this agency would be only apparent, and not real in the sense of being a special entity. It would be a necessary consequence of heredity combined with other properties of living organisms which are "internal" in the same sense that crystalline form is due to the play of internal forces, and in no other sense. If, as the recent investigations of Prof. Karl Pearson seem to show, there is a tendency on the part of a race to undergo change in a definite direction, this tendency is the necessary consequence of correlation between fertility and other characters of the organism. To say that the laws of inheritance are the expressions of a preconceived Plan may be a statement of pious opinion, but as a scientific explanation it is quite devoid of value.

R. MELDOLA.

THE TIDES POPULARLY AND PROPERLY TREATED.

The Tides and Kindred Phenomena in the Solar System. By Prof. G. H. Darwin, Plumian Professor of Astronomy, Cambridge. Pp. xviii + 342. (London: John Murray, 1898.)

WHEN a man of unequivocal scientific eminence lays aside the technicalities which have assisted him along the path of important investigations, and attempts to reveal as much as may be of his subject to the wide public who cannot understand mathematical processes, the result is certain to be at least interesting. And especially is this so in the case of a subject at once so fascinating and so perplexing as that of the tides. A phenomenon of such evident significance in the economy of the globe, of such important influence on the interests of maritime communities, must necessarily have been under observation from the earliest times. Naturally the conscientious pre-Newtonian philosopher could not do more than recognise a more or less indefinite connection between the periodic alternations of sea-level and the positions of the moon and sun. And if his mind happened to be of that type which trusts more readily to speculation than to accurate observation, his theories were even less enlightened in a corresponding degree. Even after the genius of Newton had laid a foundation of rational hypothesis, the theory which remained with little modification or development until a comparatively short time ago, was one which on many essential points was absolutely contradicted by facts. Now this is the theory of which a rather inadequate description is included in some popular works on astronomy, whose most conspicuous failing in general is that they attempt to cover a far wider range than is really practicable. However that may be, a short chapter in a work of this character was practically the only place where information on tidal phenomena was to be found in a popular form, with the exception, of course, of Lord Kelvin's admirable popular lectures. In consequence, the subject of the tides is perhaps the one about which, more than any other, the most widespread misapprehension exists, even among persons who are otherwise fairly well informed. The present work therefore fills a manifest need, and Prof. Darwin is certainly right in thinking "that there are many who would like to understand the tides, and will make the attempt to do so, provided the exposition be sufficiently simple and clear." His dictum, that "a

mathematical argument is, after all, only organised common sense," is indisputable, but so far from making the task undertaken in any way easier, it really emphasises the enormous difficulty. But Prof. Darwin has avowedly taken pains to render an intricate subject intelligible, and it will probably be generally agreed that he has achieved an unqualified success.

In publishing in book form the lectures which were delivered last year at the Lowell Institute, Boston, Massachusetts, the author has been distinctly well-advised to recast their form, and to eliminate all traces of the lecture-room. It is surely evident that there is one style appropriate to the platform and another to the essay, and that the two are of necessity mutually incompatible. It appears very unfortunate that the habit of publishing lectures in the form in which they have been delivered is becoming so prevalent. The result is that a little trouble is saved on the part of the author, always more or less to the detriment of his work. Is it easy, for example, to imagine a more irritating book than Tyndall's "Sound"? No doubt that is an extreme case, but the personal form of address is always objectionable to the reader, and ought to be eliminated.

As soon as one begins to examine the book in detail, one is struck by the excellence of Prof. Darwin's judgment in the choice and arrangement of his subject-matter. A liberal and comprehensive interpretation is placed upon the scope of tidal and kindred phenomena, so that a wide field of recent investigation is surveyed. But cognate branches and developments are always displayed in due order and significance of relation, and digressions, as they may appear to be at first sight, will be found in reality to be in perfect harmony with a continuous purpose.

The book begins where physical inquiry ought always to begin, in methods of observation: this course has the further advantage that the reader is not dismayed by difficulties at the outset. The construction and use of gauges for recording marine tides having been explained, the study of the changes of level in lakes is introduced in the second chapter. These Seiches, as they are called, constitute a distinct and exceedingly interesting phenomenon, which has not hitherto received the recognition it merits. Most readers, we fancy, will find much that is novel in this account. Dr. Forel's work on the subject was begun about a quarter of a century ago, but his researches, carried out on Lake Geneva with remarkable skill, have only been imitated elsewhere within the last year or two. There can be no doubt that highly important results will follow from the systematic application of Forel's methods which has been begun on Lake George, in New South Wales, and on the great lakes of North America. Not only will an appreciative description of Dr. Forel's work with those instruments of his own invention, the Plemyrameter and the Limnimeter, be found in this chapter, but also an excellent account of Mr. F. Napier Denison's application to this case of Helmholtz' theory of the waves generated at the surface of separation of two layers of fluid in relative motion. In dealing with the peculiar behaviour of the waters in tidal rivers, Captain Moore's work in observing the "bore" on the Tsieng-Tang-Kieng is described, and illustrated by reproductions from photographs.

At this point Prof. Darwin introduces an historical sketch, in composing which he has had the happy idea of levying contributions from his colleagues at Cambridge in the form of extracts from the early philosophical and mythological writers of such nationalities as Chinese and Arabic. Only after so much by way of introduction does he attack the mechanical theory. Admirably lucid chapters deal with the statics of the tide generating force and the deflection of the vertical. This provides an opportunity of describing his own researches with the bifilar pendulum on lunar gravity, of which this popular account is most welcome. This in turn leads to a short discussion of those seismological problems which are now attracting wide attention. The distortion of the earth's surface is also discussed as a disturbing factor in the problem of the direct measurement of the tidal force. The famous equilibrium theory of the tides is next examined, its value being insisted on as a statement of the statical conditions of the problem. But Prof. Darwin, unlike most of his predecessors among the popular writers on the subject, does not stop at this unsatisfactory stage, but goes on to consider the hydrodynamics of the tide-wave. Prof Darwin's treatment of free and forced waves in canals in different latitudes, and of tides in lakes and land-locked seas, leaves no ground for criticism, although it is impossible but that the unmathematical reader will find great difficulty in following the reasoning which the mathematician apprehends through the medium of differential equations. Any one who has fairly well mastered the foregoing chapters should have little difficulty in understanding the one in which Prof. Darwin expounds the great modern method of harmonic analysis, though here again it is scarcely possible for any but the mathematician to realise that this powerful theory leads to a unique solution of the problem, or, in other words, that it has any sounder foundation than juggling empiricism. A clear insight ought to be gained into the method by which tidal observations are reduced and made to provide the raw material for the tide-predicting machine. The practicability of tide prediction having been realised, it only remains to discuss the degree of accuracy which has been attained. This naturally leads to a consideration of the discrepancies, and the effects of wind and barometric pressure and the variation of latitude are discussed as disturbing factors. Thus the end is reached of that section of the book which deals with the subject in its more direct aspect.

It is impossible here to give any detailed account of the remaining contents of the book, although they are of absorbing interest and eminently capable of popular treatment. In masterly chapters Prof. Darwin examines the effects of tidal friction, and discusses those particular cases in which they seem to have been most clearly manifested. The chapter on the possible equilibrium figures of a rotating mass of liquid is a good example of the author's judgment in the arrangement of his material, beginning as it does with the more easily understood experiments of Plateau, and leading up to Poincaré's remarkable work. The Nebular Hypothesis as given here in outline by Prof. Darwin does not strain the limits of conceivability as it commonly does when expounded by less able writers, but is brought well within the bounds of rational probability. An excellent summary of those

researches which have led to a definite and final conclusion as to the constitution of Saturn's rings, brings the whole work to a fitting termination.

The diagrams and illustrations, though not very numerous, are generally good and clear, with the possible exception of Fig. 23, which seems to need more explanation than is given. The bibliographical notes, which are appended to every chapter, give most valuable and copious reference to the original authorities. Prof. Darwin's book ought to be read by "all those whose minds are in any degree permeated by the scientific spirit" (preface): it is certain to excite the interest and appreciation of all such, and not least of the mathematician, in spite of the fact that there is not a mathematical symbol from one end to the other.

W. E. P.

FLORA OF ROUMANIA.

Conspectul Florei Romániei. By Dr. D. Grecescu. Pp. xvi + 835. (Bucharest: Tipografiea Dreptatea, 1898.)

THIS is a valuable addition to our knowledge of the floras of the Balkan States. Prof. Grecescu's book supersedes Branza's "Prodromul Florei Românieí," which so far has been the only comprehensive work on the flora of Roumania. Branza enumerated 2100 species, of which Grecescu admits 1875 as "good." These figures refer to Roumania, exclusive of Dobrudsha. Grecescu includes, of course, the latter, and records 2450 species besides 550 varieties. This very considerable increase is partly due to the addition of the Dobrudsha flora, partly to the admission of not a few of Schur's very questionable species and of other "species minutæ," but mainly, no doubt, to the more complete collections which were at the author's disposal. The author evidently worked under considerable difficulties. He had not only to accumulate the bulk of the material on which his work rests, but was also obliged at the same time to build up, as it seems, a general herbarium of European plants for comparison. It is only fair to mention this in order that we should not criticise too severely shortcomings which are inevitable under such conditions.

The book consists of two parts, of which the first contains the Conspectus proper, or the enumeration of the species found within the borders of the kingdom of Roumania, preceded by a synoptical table of the classes and orders; whilst the second part deals with the general physiography of the country and the principal vegetations and floras of Roumania. The author follows in the arrangement of the orders on the whole the system adopted in Nyman's "Conspectus Floræ Europeæ." Why he deviates from it in certain cases is difficult to understand, if it is not partly to suit his key of orders; but when he subdivides, for instance, the "Embriogene Dicotyledóne Apetale Unisexuate" (*i.e.* the unisexual Apetalæ) into "Angiosperme" and "Gymnosperme," including Gnetaceæ in the former, then he shows such a disregard of modern nomenclature and the results of modern taxonomy, that at least an attempt of explanation ought to have been made. In fact, it is always precarious to introduce taxonomic reforms of a higher order into local floras, and the author would have done far better if he had stuck right through to Nyman's "Conspectus."

The introductory chapters of the first part ("Clasificatiea generală" and "Dispositiea familiilor naturale") are altogether the weakest part of the work, and might have been just as well omitted as being outside the scope of the book. The same applies to the short diagnoses of the tribes, subgenera and more subordinate groups which are dispersed through the enumeration of the species. As neither the genera nor the species are diagnosed, the result is an imperfect key which is useless to the beginner who does not know the genera, whilst the more advanced student who knows them is equally puzzled, as it does not carry him far enough.

The terminology is sometimes rather loose; for instance, when the perianth of Plumbagineæ and Primulaceæ is described as "herbaceous," or the terms used are obsolete, *e.g.* when "perisperm" is applied, as it was originally by Jussieu to albumen generally. Other errors, as the description of the capsules of Primulaceæ as pyxidia generally, are evidently mere slips. On the other hand, innovations like the subdivision of Gramineæ in two tribes, Eugramineæ and Maydeæ, are quite unjustifiable.

The author distinguishes three principal zones of vegetation in Roumania, *i.e.* an Alpine zone, a forest zone and a steppe zone, and he considers Roumania as forming part of a greater and natural phytogeographical region, the region of the Flora Dacica, with the Southern Carpathians as the principal focus, and extending to the Theiss in the west, the Dniester in the east, and the Danube and the Black Sea in the south and south-east. This section of the book is of considerable interest, and it is to be regretted that the author has not accompanied it by a *résumé* in French, English or German. We are sure there are many botanists who are interested in the constitution and differentiation of the Roumanian flora, but to whom a book written in Roumanian does simply not exist. They would certainly be thankful if the author would publish a translation or a comprehensive abstract of the second part of his book in one of the languages mentioned. Either, we venture to suggest, would gain very much by a careful revision which will convince him that, for instance, the number of endemic species admitted in the Alpine zone of the Flora Dacica (fully 31 per cent.!) is far too high, or that many of the so-called Mediterranean elements can hardly claim this designation.

O. STAPF.

OUR BOOK SHELF.

De Danske Barkbiller (Scolytidae et Platypodidae Danicae). By E. A. Lövendal. Pp. xii + 212; plates 5. (Copenhagen, 1898.)

IN this work Mr. Lövendal, of the Copenhagen Museum, has written a most complete account of the Danish species of bark-beetles, a subject previously dealt with by him, principally with regard to its systematic side, in Meinert's "Entomologiske Meddelelser." No European family of beetles, relatively to its size, has given rise to a more copious literature, chiefly because of its important economic relations; and in Eichhoff's "Die Europäischen Borkenkäfer," we possess already an excellent treatise on the European species known at the date of that work.

While the present monograph follows, almost inevitably, the lines of Eichhoff's work, to which it constantly refers, it is, so far as its extent goes, a great advance on that book; for the author has spared no pains in achieving an exhaustive treatment of the subject, both by his own observations and by collation of what has been written by others.

Mr. Lövendal is artist as well as author, and is already well known as the illustrator of Schiödte's "De Metamorphosi Eleutheratorum." He has executed for the present work five plates, by the now almost disused method of line-engraving, which cannot be surpassed for beauty of style or accuracy of detail. The text is furnished with some eighty woodcuts, showing the burrows of these insects in bark and wood, and the whole book is printed in the most sumptuous manner.

Such a book, written entirely in Danish and on about fifty species of a single Coleopterous family, is for the very few. But it is worthy of more general examination as a monograph which in method, fulness and finish, leaves nothing to be desired. The publication of a work of this calibre would be, we fear, at present entirely beyond the resources of British entomology. W. F. H. B.

Through Arctic Lapland. By Cutcliffe Hyne. Pp. xi + 284. (London: Adam and Charles Black, 1898.)

WE have in this volume a very interesting account of the author's journey from Vardo Island, lying in the north-east corner of Norway, to Haparanda, in Sweden, situated at the head of the Gulf of Bothnia. Mr. Cutcliffe Hyne was accompanied by his friend Mr. Cecil Hayter, to whom he is indebted for the illustrations, which form an attractive feature of the book.

On arriving at Vardo, inquiries were made as to the best means of getting across the country, and it was found that the journey was an unheard-of undertaking in the summer months, the country being chiefly swamps, lakes and rivers. In winter it would have been comparatively easy, for the ground being hard, and the water frozen, there were recognised routes, and stations where relays of deer could be obtained.

However, the travellers were not to be daunted by the apparently hopeless look-out presented to them, and they persevered, with the result that they accomplished what they set out to do.

The journey is of particular interest, for this special route had never been taken before. The incidents described are numerous and exciting, and the life and customs of the nomad Lapp are well depicted. The chief means of subsistence of this Lapp is in the possession of large herds of deer; for not only does their milk, which is thick and syrupy, form part of his daily food, but he breeds them, and rears them for selling and killing. With regard to sledge-deer, it takes three years of severe training before they can safely be driven.

All through the book we are struck with the descriptions of the beauty of the vegetation, and also with the lack of wild life. The troublesome swarms of mosquitoes and flies form a special drawback for travelling in that part. Nothing of great scientific importance is disclosed, but much which will help those who wish to visit Arctic Lapland.

The New Gulliver. By Wendell Phillips Garrison. Pp. 51. (Jamaica, Queensborough, New York: The Marion Press, 1898.)

THIS is an amusing fantasy in which a shipwrecked graduate of Yale College is supposed to be cast upon an island, and to hold dialogues with a dapple-grey horse, which refused to acknowledge Prof. Marsh's *Orohippus* or *Eohippus* as its ancestors, and explained that there could be no moral sense without language; from which conclusion certain theological distinctions are drawn.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

Converse of the Zeeman Effect.

I HAVE not seen it noticed that a converse action to the Zeeman effect should exist. A radiating atom in a magnetic field gives out circularly polarised light. A circularly polarised beam of light should cause a directed rotation of the electrons, so that the absorbing gas should be magnetised and exhibit magnetic force. If all the molecules in a c.c. of gas were caused to rotate their electrons in the same direction, it would possess quite a considerable magnetic moment. It is very improbable that the action of a circularly polarised beam of light would control the motions to such an extent as that; but it is quite possible that, if a circularly polarised beam of sunlight were passed through a strongly absorbing gas, it would magnetise it to an observable extent. The same effect would probably exist in any medium in which absorption was principally due to syntony and not mostly due to viscous actions. Hence I would expect some effect with absorbing substances like fuchsine. It is doubtful whether lampblack, iron, or other metals have a sufficiently syntonous absorption to exhibit the effect.

My assistant Mr. Thrift is engaged in trying the experiment, but in the meanwhile I thought it might be of general interest to point out that such an effect should exist.

GEO. FRAS. FITZGERALD.

Trinity College, Dublin, December 29, 1898.

Flow of Water.

I TOOK occasion, in the course of a paper on "The Character of Fluid Motion," read on December 14 before the Liverpool Engineering Society, to give the following reply, which I promised to Prof. Osborne Reynolds's letter, which appeared in your issue of September 15 last.

"Prof. Reynolds's comments may be placed under three headings:

"(1) An expression of disagreement with what he takes to be my views on the subject of the light border.

"(2) An explanation of the light border or bands adjacent to the solid boundary.

"(3) The expression of the belief that the water charged with air bubbles does not in any way represent the motion of the fluid itself.

"In the first place the criticism of Prof. Reynolds is based, apparently, upon a misconception of the statement in my first paper (*Trans.* Inst. Naval Architects, vol. xxxix. p. 151).

"In his letter he states as my views 'that with water in sinuous motion and air bubbles as indices of the manner of motion, the light bands adjacent to the surfaces of the solids, which show absence of bubbles adjacent to the solid, prove that the once air-charged water has not been carried by sinuous motion sufficiently near to the solid surface to displace the initially adjacent water; and hence prove that the sinuous motion does not extend up to the solid surface.' What I really wrote was, however, very different to this, viz. that the result of my observations had led me to the conclusion that the 'clear border line represents a condition of parallel flow of layers of water past the skin of the obstacle, or the sides of a pipe, in which a state of shearing exists, while outside this, in the darker portion, the water is in a state of sinuous motion, which corresponds to the state of the higher velocity of water.'

"These two statements are really very different because it was not my own belief that the once air-charged water never reached the surface, but that when it did so, the air has been removed from it for reasons which I gave (*Trans.* Inst. of Naval Architects, vol. xl. p. 45), where I stated that although I had 'purposely avoided introducing unnecessary speculations in trying to account for the observed facts, it does, however, seem that the clear film may be partly accounted for as the result of inertia, which allows the heavier water to reach the side of the submerged body, and partly from the fact that the velocity being less there, the pressure might be greater, and so the air being excluded from the portion where the water is moving with

parallel motion; whereas, it is entangled and broken up in the portion where the sinuous motion of the water occurs.'

"It will be observed that this explanation is to a certain extent the same as that subsequently given by Prof. Osborne Reynolds in the letter to NATURE, from which I have quoted, and to which I refer under heading (2).

"The experiments of Prof. Reynolds, which he cites himself, are entirely different from those of which I have given an account, and seem to me to have but very little bearing upon the behaviour of water in the conditions under which my experiments were conducted.

"With regard to the differences of state in the water in the light band, I will not trespass on your time with a repetition of the arguments which I have already published, and which have not hitherto been answered, but I would point out that the experiments you have seen to-night form a most striking method of putting the theory to the test by actually placing liquid under the condition of the thin border, and in obtaining when this is done, results which are absolutely different from those which were given by the thick film or sheet of water. This is one of the strongest possible arguments in favour of the views which I have advanced, inasmuch as the results of the experiment seem to have surprised some of the greatest authorities on the subject.

"Coming to (3), I would first remark, though it has never been distinctly claimed, that the water charged with air actually represented real stream-line motion, in my first paper it was stated that 'there was no difference whether the smallest quantity of air was present, or so large a quantity as to render the slide opaque, as the general behaviour of the flow of water was practically unaffected by the presence of the air.'

"Though this remark only applied to the general nature of the results obtained, the experiments brought out very strongly, I believe for the first time, various points which had long been known to the naval architect, but which had not been actually made visible to the eye. Prof. Osborne Reynolds, however, states that 'air bubbles are the most misleading bodies that can possibly be chosen to indicate the lines of motion in a fluid in sinuous motion.' This remark led me to consider the very appropriate experiment of trying the effect of first taking the air-charged water upwards and then taking it downwards under the same conditions. Inasmuch as the air in one case was trying to rise upwards through the water, which is moving in the same direction, whereas in the other case it is trying to move in the opposite direction, it is perfectly certain that if there was much difference in the flow owing to the presence of air, it would become marked under those circumstances.

FIG. 1.

FIG. 2.

"The two photographs, Figs. 1 and 2, represent the results of this experiment, and I venture to think that no one would be able to tell from the photographs themselves where the air-charged water was flowing upwards or where it was flowing downwards. This, I think, shows in a very striking way the comparatively small effect which the presence of air under *suitable* conditions, has upon the flow of the water, and it is quite contrary to that which Prof. Osborne Reynolds seems to anticipate."

The truth of the whole matter appears to be this, that as far as Prof. Osborne Reynolds has dealt with the behaviour of air in water, it has been under conditions represented by plates suddenly immersed or moving through still water, in which case air has been in a state of bubbles of large size, whereas in my own experiments, the air has been broken up into very minute bubbles. The behaviour in the two cases has been as different as that of a mass of water, say from a bucket, thrown through the air, and the finely-divided particles of moisture in a fog or mist. The effect of viscosity makes all the difference in the two cases. As, however, I have never had an opportunity of seeing Prof. Osborne Reynolds's experiments, and as he has never witnessed mine, perhaps his disagreement with me is, after all, only another illustration of the old fable of the chameleon.

H. S. HELE-SHAW.

University College, Liverpool, December 21, 1898.

Etherion, a New Gas?

SOME months ago the discovery of a new gas, by Mr. C. Brush, was announced by nearly all the scientific periodicals of the world, which was said to be endowed with quite extraordinary properties; as, for instance, density one ten-thousandth of hydrogen, molecular velocity and heat conductivity hundred times that of hydrogen!

It seemed strange to me that such tremendous assumptions should be based on no more convincing arguments than the experiments (Mr. Brush's) reported therein, on the relative increase of condition of heat in rarefied air, when glass powder, contained in the same vessel, was being heated, and I was waiting eagerly for the publication of the original account (Mr. Brush's), since it struck me that all observed phenomena could be explained by the well-known properties of water vapour.

I was glad to learn afterwards, from a paper in the *Chemical News* (November 4, p. 221), that this is also the opinion of Sir William Crookes, undoubtedly the greatest authority in this kind of research.

Now the original paper (Mr. Brush's) appeared in *Science* for October 14. It has been already the subject of a severe criticism by "A Physicist" in the *Chemical News* (December 2, p. 277), as it does not contain indeed any further argument for Mr. Brush's hypothesis. I do not think it superfluous, however, to warn in NATURE, too, against an excessive credulity in this matter, and to point at some facts not yet emphasised sufficiently by other sceptics.

First I must mention, for the sake of those readers who are not sufficiently acquainted with these things, that the conduction of heat by gases, when not disturbed by convection currents, is independent of the pressure, until this comes down to several millimetres of mercury; then it begins to decrease, at first very slowly, then faster, until it becomes nearly proportional to the pressure, at the highest rarefactions; and the differences of conduction in various gases, very marked at higher pressures, are much less at pressures of several millionths of an atmosphere.

These facts, which are in strictest accordance with the kinetic theory of gases, as I have shown in the *Phil. Mag.* (August 1898), have been investigated besides by other observers very carefully, also by Mr. Brush himself, and form the object of a very interesting paper of his in the *Phil. Mag.* (January).

Mr. Brush found that the conduction of heat is increased very much at high exhaustions in respect to other gases at corresponding pressures, when glass powder is heated (but remaining always much smaller than at normal pressures), and he infers from this that a gas of enormous conductivity is given off by heated glass powder.

Now to me the main point seems to be—how did Mr. Brush measure these low pressures? By an improved form of the Macleod gauge, which seems to be very suitable for dry gas, but of course, like every Macleod gauge, is quite unfit for gases where moisture or other condensable vapours are present, since then it indicates only the partial pressure of the not condensable gas. He did not use any drying agents, since they absorbed the gas in question, therefore the indications of the gauge are of no value whatever as to the total gas pressure.

When Mr. Brush measured a gas pressure of 0·38 millionth, and found the conduction to be about forty times that of dry air at this pressure, he may have had in reality a pressure of 0·38 millionth of air, and besides a pressure of 20 millionth of

water vapour, which of course would be sufficient to account for that large effect.

Sir William Crookes mentions that, according to his experiments, water vapour produces a greater conduction at very low pressures than air; but I decidedly disbelieve the difference to be in any way similar to the numbers given by Mr. Brush, in consequence of a reasoning mentioned at the end of my *Phil. Mag.* paper, and I think the simplest explanation to be afforded by this source of errors vitiating the pressure indications.

It is no mere hypothesis but a certain fact that water vapour is being evolved by heated glass, and probably many other substances. Sir William Crookes gathers quite a number of arguments for it from his own researches, amongst others spectroscopical proofs. The same opinion was put forward by Kundt and Warburg, who were led to it by the very same sort of experiments on conduction of heat as Mr. Brush's, which they made as early as 1875 (*Poggendorff's Annalen*, 156, p. 177). Further investigations on the hygroscopical properties of glass, and on means for partially removing them, were published by Warburg and Ihmori (*Wiedemann's Annalen*, 27, p. 481).

I cannot prove, of course, that there is no new gas evolved, but I maintain that whatever facts Mr. Brush has put forward as an evidence for its existence, can be explained quite simply by the presence of water vapour (perhaps also other condensable vapours), which he seems to have overlooked. I do not think it necessary to go into details, and to analyse more thoroughly the —rather fantastic—speculative part of the paper, where scarcely any statement is not open to serious objections.

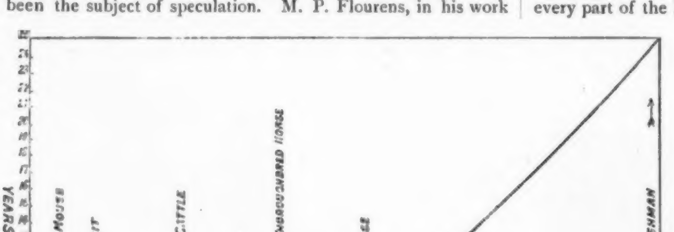
Although thus I differ from Mr. Brush very much in respect to the interpretation of his recent results, I think his elaborate experimental investigations, as reported in his *Phil. Mag.* paper, to be of great value for the theory of these phenomena; certainly it would be very desirable that he might carry on those researches, as he promised there to do.

M. SMOLUCHOWSKI DE SMOLAN.

Vienna, University.

The Curve of Life.

THE relationship between the duration of adolescence and potential longevity in different species of mammals has repeatedly been the subject of speculation. M. P. Flourens, in his work



on "Human Longevity," made the ratio between the two periods as 1 to 5; Buffon had previously concluded that it was as 1 to 7. In neither case were the data sufficiently numerous and trustworthy to make these figures generally accepted. In the course of some investigations on the variability of the adolescent period in different breeds of the same species among certain well-known mammals, I have satisfied myself that a relationship exists between the duration of growth and the length of an animal's natural life; although it is evidently not of the kind suspected by the older writers. It may be stated as follows:—The ratio of length of adolescence to length of life in the shortest lived mammals is proportionally less than it is in longer lived mammals. For example, the period of growth and development of the domestic mouse is, according to my informant, a breeder of these small rodents, about three months. Its natural lifetime is four years. In other words, the mouse may be expected to live about fifteen times its adolescent period as a mature animal. The Arab horse, according to a well-known authority, arrives at maturity in about eight years, its lifetime is about forty years; that is to say, the animal lives four times the length of its adolescence as an adult. Man, on the other hand, who only completes his growth by the union of the sternal epiphysis of the clavicle to its shaft at the age of twenty-five, has, after passing his fiftieth year, or "the middle arch of life," to use Dr. Farr's phrase, only another twenty-five years' expectation of life. His potential longevity accordingly foreshadows a period of maturity not greater than twice the length of his youth.

I have obtained, through the kindness of numerous correspondents interested in breeding and rearing of farm and other domestic animals, the approximate lengths of these two periods in a few well-known mammals; and the accompanying diagram shows the relations between growth and longevity among the same animals plotted as a definite curve. This result was entirely unexpected by me, and it may be interesting to some of your readers.

W. AINSLIE HOLLIS.

Hove.

The Alleged Destruction of Swallows and Martins in Italy.

IN your issue of December 22, 1898, I read the report of a conference held by the Society for the Protection of Birds, at which a paper was read on the decrease of swallows and martins coming to England, giving, as a reason for this decrease, the netting of thousands of these birds on their arrival at the Italian shores, and their subsequent consumption as food.

May I venture to remark that, during a residence of some years in Italy, I have never once seen a swallow, or any member of its family, exposed for sale, and that I have never known, or heard of, an instance of their being netted in the manner described, though I am well acquainted with nearly every part of the Peninsula.

Since reading the above mentioned accusation against Italy, I have asked several Italians whether they knew of such a practice, and am informed that it is simply non-existent, the swallow being, perhaps, the one bird in this country which is regarded with a kind of sentiment by all classes, as the harbinger of spring.

Swallows, moreover, do not arrive on these coasts in a state of exhaustion, and to net them would be no easy feat.

A few isolated cases of the cruel method of capturing them with artificial flies may occur, but not more so than in England.

The Italian may be ruthless in his destruction of other birds, but is certainly not a destroyer of the *Hirundinidae*.

Of the similar charge made against the French, I am not in a position to judge; but I imagine that the cause for the decrease of the *Hirundinidae* in England may lie in quite another direction, and may be attributable to some equivalent decrease of their favourite insects in our islands, or in some atmospheric and climatic change. Italy, I am convinced, is not responsible in any way for it.

RICHARD BAGOT.

Roma, December 27, 1898.

RADIATION PHENOMENA IN THE MAGNETIC FIELD.

IN the spring of 1897 the scientific world became indebted to Dr. Zeeman for the observation that when a source of light is placed in a strong magnetic field the spectral lines of the light emitted by that source suffer

marked modification. The general type, or characteristic type, of this modification is that when the slit of the spectroscope views the sources of light across the lines of magnetic force, each spectral line becomes a triplet, of which the middle line has the same wave-length as the original line; whereas the side lines of the triplet have wave-lengths, respectively, a little longer and a little shorter than that of the unmodified line, the difference of wave-length being proportional to the strength of the magnetic field. Further, the central line has its vibrations parallel to the lines of force, whereas the side lines of the triplet have their vibrations perpendicular to the lines of force. Thus, if the axis of the magnetic field is horizontal, so that the lines of force are horizontal, and if the slit of the spectroscope looks horizontally across the lines of force, then in the central constituent of the triplet the vibrations are horizontal, while in the side lines the vibrations are vertical. Thus the central line is plane polarised, and the side lines are also plane-polarised, but in a perpendicular plane. This is the typical phenomenon when the light is viewed across the lines of force. When the light is viewed along the lines of force—that is, through axial holes pierced in the pole-pieces of the electro-magnet, the modification is different. In this case, instead of a triplet with plane polarised constituents, we are presented with a doublet, having circularly polarised constituents. That is, each spectral line is broken up into two lines of slightly different wave-length; one constituent being circularly polarised in one sense, and the other in the opposite sense. As before, the difference of wave-length, and therefore the separation of the constituents of these doublets in the spectroscope, is proportional to the strength of the magnetic field for each line, but differs in amount for the different spectral lines.

In order to fix the ideas of those who are not familiar with this department of physics, the phenomena described above are represented diagrammatically in Fig. 1. Thus at A the upper single line is supposed to represent a bright spectral line of some substance when the radiating source is not influenced by the magnetic field. This line becomes converted into three distinct lines, that is a triplet, as shown underneath at A′, when the source of light is subject to a strong magnetic field, and the radiation takes place across the lines of force. If N be the vibration frequency of A, then the vibration frequencies of the members of the triplet A′, into which A is converted, are N − n, N, N + n, where N is a small quantity depending on the strength of the magnetic field. On the other hand, when the source of light is viewed along the lines of force a bright spectral line, B, becomes converted into a doublet, B′, consisting of two distinct lines which are circularly polarised in opposite senses. The constituents of the triplet A′ are, on the contrary, plane polarised, the direction of vibration in the middle line being horizontal, while that in the side lines is vertical.

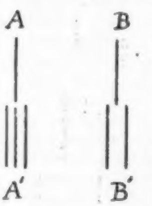


FIG. 1.

The foregoing are the phenomena demanded by the simplest form of theory, and they are the phenomena actually yielded by experiment in the case of the vast majority of spectral lines. Many lines, however, when carefully examined in a sufficiently strong magnetic field, yield phenomena which differ in a remarkable manner from the simple theoretical expectation described above. In some cases the middle line of the triplet becomes resolved into a pair of lines so that the triplet becomes a quartet, while in other cases each line of the triplet becomes a pair, and thus a sextet is produced; and in some cases the side lines of the triplet become resolved into triplets, while the middle line becomes a doublet, and then an octet is produced, and so on. Thus generally, when the light is viewed across the lines of force, we may say a single spectral line becomes resolved by the magnetic field into a system of lines consisting of a central part bordered by two side parts. The central part may consist of one or more lines, and is plane polarised, while the side parts may each consist of one or more lines, and are also plane polarised in a plane at right angles to the plane of polarisation of the central part.

On account of this opposite polarisation the central part may be quenched and the sides examined separately, or *vice versa*, by means of a nicol's prism, and consequently the existence of this plane polarisation enables us to scrutinise the phenomena much more closely and effectively than would be otherwise possible unless, indeed, a magnetic field of any desired strength could be produced so as to obtain complete and wide separation of the various constituents of the modified line. But it is not possible at present to produce a magnetic field for working purposes of a strength exceeding 30,000 to 40,000 C.G.S. units. Hence the polarisation is of importance for purposes of observation. The best way to take advantage of it is not to use a nicol's prism (which lets through only one of the two plane polarised beams), but to use instead a double image prism, or a rhomb of doubly refracting crystal, placed before the slit of the spectroscrope, so that two images of the source are produced on the slit, one above the other (the slit being supposed vertical). Of these images one consists of light vibrating horizontally—that is, it consists of the light which forms the central part of the triplet A′ (Fig. 1), while the other image consists of light vibrating vertically—that is, the light which forms the sides of the triplet A′ when the magnetic field is excited. These two images on the slit give rise to two spectra in the field of view of the spectroscope, one above the other: one consisting of the lines which form the centres of the triplets, and the other of the lines which form the sides. This is shown in Fig. 2, where A represents a triplet as seen in the field of view, without the use of any nicol or double image prism, and A′ represents what is seen when a double image prism is used. The upper line in A′ represents the light vibrating horizontally, and is what would be seen if a nicol's prism were placed before the slit with its principal plane vertical; whereas the two lines below in A′ are formed by the light vibrating vertically, and constitute what would be seen were the nicol turned through a right angle. With the double image prism, however, the upper and the lower lines in A′ are seen simultaneously, and so a great deal of trouble is avoided, and much time is saved when the phenomena are being photographed. But the chief advantage of separating the middle from the side lines, as at A′ (Fig. 2), lies in the fact that in many cases the difference of wave-length of the middle and the side lines is so small, even in a very strong magnetic field, that the width of the lines causes them to overlap, and so obliterate the phenomena. It was for this reason that in the earlier experiments made by Dr. Zeeman, merely a broadening of the spectral lines was observed, and not a tripling. In fact, it was not until theory pointed out that tripling and plane polarisation should exist across the lines of force, that Zeeman interposed a nicol's prism, and found that the broadened line exhibited the polarisation required, and that the facts were not discordant with the theory. It is to be observed, however, as I have pointed out elsewhere, that the removal of the central part from the broadened line by a nicol properly interposed (so that

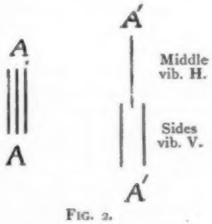


FIG. 2.

the broadened line now appears as a doublet), does not absolutely prove that the broadened line is a triplet with its components overlapped. It merely determines that the broadened line may be a triplet, and that the theory which anticipates the tripling may be correct. In order to place this matter beyond all doubt, it is necessary to so increase the strength of the magnetic field that the components of the triplet (if they exist) shall be completely separated from one another; and when this[1] is done, it is found that the tripling exists, but it is also found that many divergencies from the uniform expectation of theory (pure tripling) exist. Thus, as pointed out above, many lines under the influence of the magnetic field show as quartets, or sextets, or octets, or other modified form of the normal triplets. In the examination of these cases the double image prism forms a very valuable adjunct, as all the light polarised in one plane goes to form one image, while all the light polarised in the perpendicular plane forms the other image. The appearance presented in the field of view of the spectroscope by different types of lines, under these circumstances, is shown in Fig. 3. In this figure the lines of the upper row are formed by one image from the double image prism—that is to say, by the light vibrating horizontally, and correspond to the central members of the normal triplets; while the bottom row consists of light vibrating vertically, and represents the side lines of the normal triplets. Thus at A A′ we have the normal triplet, as expected by theory, with the central line, A, polarised in one plane, while the two side lines, A′, are polarised in the perpendicular plane. This type exists in the case of by far the greater number of spectral lines, and may be regarded as the general or normal type, if for no other reason than the frequency with which it occurs. The second type, shown at B B′, is a quartet in which, instead of a single middle line, we have two middle lines close together at B, with the two side lines at B′ as before. This type of quartet occurs in the blue cadmium line 4800, and in the blue zinc line 4722. At C C′ another species of quartet is shown; in this there are two middle lines also, but the separation of these is almost as wide as that of the side lines, so that the appearance presented to the eye when the double image prism is not used is that of two fine doublets, rather than the quartet appearance of the type B B′. This third type, C C′, occurs in the case of the sodium line D_1, the greenish-blue line of barium 4934, and many others. The fourth type, D D′, is a sextet of fine uniformly spaced lines, two of which correspond to each component of the normal triplet. That is, the central component is a doublet, and each of the side components is also a doublet. This type is represented by the line D_2 of sodium. The fifth type is shown at E E′, where the central constituent is a doublet, and each of the side components is a triplet. The distance between the components of the central doublet in this case is about the same as that between the central members of the side triplets. This type is represented in the yellow line of barium 5850. All the variations so far noted may be embraced in the general statement that each line of the normal triplet A A′ may itself become a doublet or a triplet.

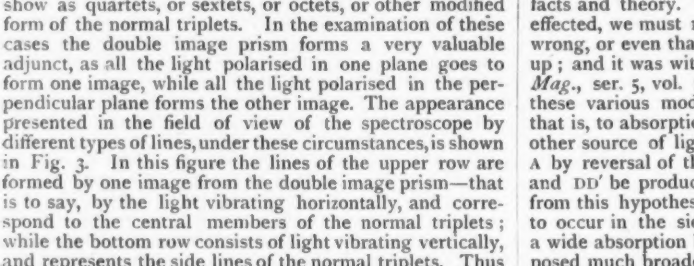


FIG. 3.

The question now of greatest importance is whether these various types of modification by the magnetic field are consistent with the theoretical explanations of the phenomena put forward by Larmor, Lorentz, and others? Naturally one must endeavour to reconcile facts and theory. If this reconciliation has not yet been effected, we must not hastily conclude that the theory is wrong, or even that it requires to be modified or patched up; and it was with this feeling that I put forward (*Phil. Mag.*, ser. 5, vol. xlv. p. 325, April 1898) the idea that these various modifications might be due to reversal—that is, to absorption in the outer parts of the spark or other source of light. Thus B (Fig. 3) might arise from A by reversal of the middle line, and so also might CC′ and DD′ be produced, and even EE′ might be intelligible from this hypothesis if we supposed double reversal (?) to occur in the side components of the triplet AA′, and a wide absorption band to occur in the middle line (supposed much broader than the others). But (as I stated when putting forward this view) the appearance presented to the eye is not that of ordinary reversal, so that appearances are against the supposition that the modifications are due to absorption in the vapour surrounding the source of light. But still it is to be remembered that the magnetic field exerts a considerable influence on the source of light, and might alter considerably the appearance of an ordinary reversal. However, in order to test this matter, I observed many lines, which deviate from the normal triplet type, in a magnetic field of gradually increasing strength. The object of this was to determine if the separation of the lines forming the upper row in Fig. 3 (say, the doublet B or C) depended on the strength of the magnetic field. Thus, if the components of the doublet B remain fixed while the distance between the side lines B′ continues to increase as the magnetic field increases in strength, then we might conclude that reversal is not only a possible explanation by very probably the true explanation. But the components of the central parts B, C, D, do not remain fixed as the magnetic field increases in strength. On the contrary, the distance between the two lines B increases as the strength of the field increases; indeed, as far as rough observations go, the distance between the components of B or C, like the distance between the side lines B′ or C′, is proportional to the strength of the magnetic field. Similar remarks apply to the types DD′, EE′, &c. When the field increases in strength, the lines forming D separate from each other, and so also do the doublets D′, and the lines forming each component of the latter also separate, so that the sextet remains a system of equally spaced lines. On the other hand, when the field is reduced in strength the various lines close up till B, C, D, E each appears as a single line with B′, C′, D′ as narrow doublets—in fact, the normal triplet type is approached in appearance as the field is reduced.

It appears, therefore, that the explanation of the various modifications of the normal triplet type cannot be satisfactorily explained by reversal, and consequently these divergencies must be referred to the action of the magnetic field on the vibrating structure which emits the

[1] This was effected by the writer in October 1897, and triplets and quartets were then photographed directly without the aid of a nicol or any polarising apparatus whatever. (See letter to NATURE, dated November 10, 1897, vol. lvii. p. 173.) These photographs were shown at the November meeting of the Dublin University Experimental Science Association, at the December 1897 meeting of the Royal Dublin Society, and at the January 1898 meeting of the Royal Society of London; but it was not until April 1898 that they were reproduced in the *Philosophical Magazine* (5th series, vol. xlv. p. 325, plate xxiii.).

radiation. Now the theory which indicates that a spectral line should be slit up into a pure triplet by the action of the magnetic field, assumes that the freedom of vibration is the same in all directions, and it is from this that the resolution into triplets occurs. This assumption is that which one most naturally makes in a first attack on a problem of this nature, but no one making it would be surprised if the facts did not turn out more complicated than the prediction of such a solution. For example, it is quite possible to conceive a state of affairs in which the magnetic field may constrain all vibrations to take place along the lines of force, in which case the side lines of the triplet would vanish; or, on the other hand, vibration in the direction of the lines of force might be impossible, in which case the central line of the triplet would vanish. Indeed, one is somewhat surprised that deviations of this kind from the normal triplet type do not more frequently occur. In fact, when I first examined the spectrum of iron, I hoped to find many deviations of this kind, but failed to detect any very marked difference between the behaviour of iron and other substances. This is not much to be wondered at when it is remembered that iron ceases to be magnetic at a comparatively low temperature, and, therefore, at the temperature of the spark of an induction coil, one should not expect its vapour to behave much differently from that of any other substance.

However, as already stated, the normal triplet type arises in theory because the orbit of the vibrating electron is supposed free from constraints and perturbations—that is, that movement is equally free in all directions. When constraints are imposed, or new forces arise which cause perturbations in the orbit, new frequencies will be introduced into the vibrating system. Thus if an electron, or an atom, or a particle describes an ellipse under a central force with frequency N, and if disturbing forces came into play which cause the apse line to rotate with frequency n, then, as Dr. Stoney [1] has shown, a spectral line arising from the original vibration of frequency N will become replaced by two others of frequencies $N + n$ and $N - n$ respectively. Again, if the disturbing forces cause a precessional motion of the plane of the orbit round a fixed line with frequency n, the original vibration of frequency N becomes replaced by three others of frequencies $N + n$, N, and $N - n$ respectively, and similar phenomena arise when other periodic disturbances occur in the orbital motion. We are prepared, therefore, to find that each line of the normal triplet may become itself a doublet or a triplet. [2]

The disturbing forces arising from the action of the magnetic field should increase with the strength of the field, so that if the distance between components of the doublet B or C or D or E (Fig. 3), which takes the place of the central line of the normal triplet, should increase with the magnetic field, as it is found to do by experiment. In fact, if the distance between the side lines of the normal triplet AA′ be written in the form $d_1 = k_1H$, where H is the strength of the field, and k_1 a quantity depending on the wave-length and other constants involved in the production of the particular line in question, then the distance between the components of the modified central component B, C, &c., may be written in the form $d_2 = k_2H$. Thus as the field increases in strength the whole system of lines into which any given spectral line becomes resolved, separate laterally from each other proportionately, as it were, according to a given scale. Similar remarks, of course, apply to systems like DD′ and EE′.

Now in any particular case, such as BB′ for example, if the distance between the pair of lines B′ is

$$d_1 = k_1H$$

while the distance between the pair B is

$$d_2 = k_2H$$

there is apparently no reason why k_1 should be greater than, or less than k_2. Whether k_1 is greater than or less than k_2, must be determined by the action of the magnetic field on the system which produces the particular spectral line in question. Accordingly we are prepared to find that in some lines the components of the central line, as at B, shall be much closer together than the side components at B′, while in others, as at C, E and F, the distance d_2 is nearly equal to, or may be even greater than the distance d_1 between the side lines. Thus, once the production of a quartet of the type BB′ is explained, all the other modifications become intelligible. The case in which the components F are wider apart than the side lines F′ (so that the centre, as it were, encloses the sides) is merely the same phenomenon (only more accentuated) as that shown at BB′ where the separation d_2 is less than d_1. This point is mentioned here specially because in some cases the separation d_2 is actually greater than d_1, and it seems to be regarded as a difficulty of a much higher order than that in which occurs in the ordinary quartet, where d_2 is less than d_1.

Lines of the former type FF′, viz. that in which d_2 is greater than d_1, seem to have been first observed by MM. Henri Becquerel and H. Deslandres (see *Comptes rendus*, t. 126, p. 997, April 4, 1898) in the spectrum of iron, and subsequently Messrs. J. S. Ames, R. F. Earhart and H. M. Reese announced that they had observed the form GG′ (Fig. 3) in the spectrum of iron. In this type the side lines G′ coincide, or are not sensibly separated, while the components of the central part G are well separated.[1] (see *Astro. Phys. Journal*, vol. viii. p. 48, June 1898). The form in which this observation was described was calculated to startle, if not confound, the most firm believer in theory. It was said that these lines exhibited *reversed polarisation*—that is, that the polarisation of the centre is that which should occur in the sides, and *vice versa*. Stated in this way it is rather calculated to take one's breath away, but when stated as in the foregoing, it loses all special significance, viz. that it is merely a case of d_2 being greater than d_1, that is $k_2 > k_1$, or a quartet in which the distance between the horizontally vibrating constituents is greater than the distance between the vertically vibrating constituents. Stated in this way it falls into line with the other phenomena, and is reduced to the explanation of the doubling of any one individual member of the normal triplet.

Other similar modifications have been observed by MM. Becquerel and Deslandres, who appear to have examined the spectrum of iron very thoroughly as well as the bands of carbon and cyanogen. These bands they found to be unaffected by a magnetic field strong enough to sensibly split up the air lines.

Investigations demanding special attention are those of Prof. A. A. Michelson, both on account of his reputation as an original investigator and by reason of the nature of the apparatus which he employed. Working with his interferometer, Prof. Michelson concluded some years ago (*Phil. Mag.*, vol. xxxiv. p. 280, 1892) that the spectral lines themselves instead of being, as ordinarily supposed, narrow bands of approximately uniform illumination from edge to edge, are on the contrary in most cases really complexes, some of them being close triplets, and so on. This structure has never yet been observed by means of any ordinary form of spectroscope, and accordingly it has been suggested that it does not

[1] Dr. G. J. Stoney, *Trans. Roy. Dub. Soc.*, Vol. iv Series 2, p. 563, 1891. This is a very important paper when considered in connection with the above-mentioned magnetic perturbations of the spectral lines.

[2] These matters are treated in further detail in a paper by the present writer to appear in the forthcoming number of the *Philosophical Magazine*.

[1] I have not yet observed this type, nor do my photographs verify the conclusion of Messrs. Ames, Earhart and Reese, regarding the lines mentioned by them as belonging to this type. (This is further referred to in the forthcoming number of the *Phil. Mag.*)

exist in the light radiated from the source, but is imposed on the spectral lines by the apparatus used, namely, the interferometer. Be this as it may, the application of this instrument to the study of radiation phenomena in the magnetic field is highly interesting. In his first experiments Michelson merely observed a *doubling* of the spectral lines both along and at right angles to the lines of force (!), but subsequent observations proved that tripling occurred across the field of force, and that the constituents of the triplets were themselves multiple lines (see *Astro. Phys. Journal*, vol. vi. p. 48, 1897; vol. vii. p. 131, 1898; vol. viii. p. 43, 1898). But this is accompanied by the most surprising statement that the separation of the lines in the triplets produced by the magnetic field is independent of both the spectral line and the substance. In other words, that the separation is the same for all lines and all substances! Now, in all observations with ordinary grating or prism spectroscopes the separation of the components produced by the magnetic field varies very considerably for the different spectral lines of the same or of different substances. Even in the case of lines of nearly the same wave-length the difference is often very marked. The separation not only differs for different substances, but it is some complex function of the wave-length for any one substance. That the interferometer has led to such a law as that announced by Prof. Michelson, shows that there is some peculiarity of the instrument not yet taken into account—or else that by chance Prof. Michelson has happened to confine his observations to lines which give approximately the same separation; yet this latter could not be easily done. Be this as it may, Michelson has examined these phenomena by aid of another new instrument of his own design—the Echelon spectroscope (*Astro. Phys. Journal*, vol. viii. p. 43, 1898). With this instrument he states that the results previously obtained by aid of the interferometer, and the visibility curve, were confirmed. And this is striking, for if it confirms the general law stated by him in regard to the separation of the components, then the interferometer and the Echelon spectroscope are at variance with all other forms of spectroscope.

With apparatus which reveals structure or multiplicity in the ordinary spectral lines, it is to be expected that multiplicity would be readily revealed in the constituents produced by the magnetic field; yet in the case of some lines, the amount of finer structure revealed does not appear to be as great as that observed with a good grating, and this with other discrepancies require clearing up. If we suppose that an ordinary spectral line really consists of two or more very close lines, not separated in ordinary spectroscopes, and if we suppose that this multiplicity is produced by small perturbations caused by events inside the molecule, then it is clear that the further perturbations (if any) brought about by the magnetic field, may either increase, or diminish, or possibly reverse, those previously existing in the free field. And from this point of view the following most interesting observations made by Michelson (*loc. cit.*) become intelligible. "A very remarkable effect is observed in the case of the yellow copper line. This line without the field is a close double, the distance being 1/150th of the distance between the D lines, or 0·04 A.V. As the field increases the lines merge together without broadening, and with a strong field there is but a single narrow line."

"The behaviour of the yellow-green line of manganese is even more striking. The line is a quadruple line, just resolvable. In a weak magnetic field the light accumulates in the centre of the group, the lines becoming indistinct and merging together. In a strong field the quadruple band is reduced to a single fine line at the centre of the group."

In conclusion, it is necessary to mention briefly some ingenious methods which have been devised to exhibit the existence of the Zeeman phenomena in comparatively weak magnetic fields. The first of these chronologically was devised by M. Cotton (*Comptes rendus*, t. 125, p. 865) in 1897, and depends upon the fact that if a small sodium flame, A, be placed in front of a larger one, B, and viewed against it, the outer edges of the small flame appear dark. This arises, as is well known, from the absorption which takes place in the outer sheath of the smaller flame. If, however, the flame B be placed in the magnetic field, the dark border around A disappears. This arises from the fact that the magnetic field induces new periods of vibration in B (the side lines) which are not possessed by A, and therefore not absorbed.

The next experiment to be mentioned is one of special elegance, devised by Prof. Auguste Righi (*Comptes rendus*, t. 127, p. 216, 1898, and *Rend. della R. Accad. dei Lincei*, July 1898). If a plane polarised beam of light from a powerful source, such as an arc lamp, be transmitted through an absorbing vapour, such as a sodium flame, or sodium vapour in a tube, and if the light, after passing through the vapour, be transmitted through a nicol's prism, and then received on the slit of a spectroscope, a continuous spectrum will be observed in which dark lines occur corresponding to the absorption lines or bands of the vapour. If the analysing nicol be rotated till its principal plane is perpendicular to that of the polariser, then all light in the spectroscope will be extinguished.[1] Now suppose this to be so arranged, and suppose, further, that the absorbing vapour is between the pole-pieces of a magnet so as to be subject to the action of the magnetic field, and suppose that the light passes through this vapour along the lines of magnetic force by passing through axial holes pierced in the pole-pieces, then under these circumstances, if the magnet be excited, bright lines appear in the spectroscope corresponding to the absorption lines of the vapour. At first sight it appears as if the magnetic field caused the vapour to emit its own vibrations as if it were highly luminous. It is not so, however. The explanation is that the magnetic field so affects the vapour, that if it were self-luminous any spectral line appertaining to it of frequency N is converted into two other vibrations of frequency $N + n$ and $N - n$ respectively; and these two, along the lines of force, are circularly polarised in opposite senses, and consequently the vapour when cold possesses the power of absorbing vibration of frequencies $N + n$ and $N - n$. Now the beam from the electric arc passing through the vapour being continuous, possesses vibrations of frequency $N + n$ and also of frequency $N - n$. These vibrations in the arrangement, described above, are plane polarised, and any plane polarised vibration is equivalent to two opposite circular vibrations. The result is that the vapour absorbs one of the circular components from the rectilinear vibration $N + n$, and transmits the other. In the same way it also absorbs one of the circular component vibrations from the vibration $N - n$, and transmits the other. These transmitted circular components are very intense (having evidently half the intensity possessed by the arc light), and they cannot be extinguished by the analysing nicol, so they consequently appear in the spectroscope. If the magnetic field is not very strong, the vibrations $N + n$ and $N - n$ practically coincide with N, and what is presented to the eye is that the absorption lines of the vapour become bright when the magnetic field is excited. This can be observed in fields of very small intensity.

Prof. Righi mentions that the phenomenon observed in the foregoing experiment does not occur when the light traverses the vapour in a direction perpendicular to the lines of force. This is a result which differs from

[1] If a sodium flame be used as the absorber, then of course faint sodium lines will still remain. For this reason the sodium flame used should not be bright.

the theoretical expectation. For an emission frequency N of the vapour will now be converted, across the lines of force, into absorption frequencies $N+n$, N, and $N-n$. The first and last being for vertical vibrations, and the central one for horizontal vibrations. If, therefore, the plane of polarisation of the incident light (arc lamp) be inclined at any angle a to the vertical, its horizontal component will be absorbed by the vapour for the frequency N, and its vertical component for the frequencies $N+n$ and $N-n$. The other components will be transmitted, and being vertical and horizontal respectively, and not being of the same period, they cannot be extinguished by a nicol set to quench light polarised at an angle a to the vertical. When the incident light is polarised in a vertical plane, however, or in a horizontal plane, the analysing nicol can quench the transmitted light, and the lines do not light up in the spectroscope. The writer has found on trial that the expectation of theory is realised, and that when the polariser is inclined to the vertical the phenomenon takes place across the lines of force as in Righi's experiment along the lines of force.[1]

Many other interesting points deserve notice, such as Prof. G. F. Fitzgerald's theory connecting the Faraday effect with the Zeeman effect; but want of space compels us to close the present account of the work done in this field during the past year. We may just mention, in conclusion, that the Faraday effect in gases has been placed in strong evidence by an interesting experiment due to MM. Macaluso and Corbino (*Comptes rendus*, t. 127, p. 548, 1898), which depends for its explanation on the fact that the rotatory power of a substance increases enormously as the frequency of the transmitted light approaches that of an absorption band of the substance through which it is transmitted.

THOMAS PRESTON.

RECENT WORK IN COMPARATIVE MYOLOGY.

THE introduction of biology into medical education, productive of such good effects, has been in few departments more beneficial than in that of comparative anatomy; and we desire to call attention to the present aspect of one outcome of this, which appears to us a direct result of the adoption by the surgical anatomist of the comparative method, and to be full of promise for the future.

Our remarks are prompted by a paper which has recently appeared in the *Proceedings* of the Zoological Society, as the completion of an extensive study of the comparative myology of the terrestrial carnivora, by Prof. Bertram Windle and Mr. F. G. Parsons, a most laborious piece of work, involving the careful dissection of close upon one hundred individuals representative of the leading carnivorous families. Of desultory descriptions of the myology of individual mammals we have long had enough, and it is the merit of the two anatomists named to have attacked the subject in a thoroughly systematic manner, transcending that of most of their predecessors. Incorporating with their own extensive observations the sum of our previous knowledge, by carefully classifying the muscles, dealing with them in sets, and tabulating their relationships where necessary in a manner permitting of ready reference, they have now laid the foundation of a really comprehensive system of recording myological facts. The paper to which we refer is serial with others which its authors have published on the Rodentia and Marsupialia, either individually or together; and perusal of the series leads us to believe that in finally deciding the zoological position of some of the more anomalous mammalian forms the study of the muscles may yet play a not unimportant part. We welcome thus, for example, the conclusion of one of the authors that the Jerboas are allied to the Myomorpha as insisted upon by Winge; and the fact that in other respects they are in agreement with his recent work in the classification of the Mammalia, the value and importance of which has been by no means sufficiently appreciated in this country.

In having access to the collections of the Zoological Society, Royal College of Surgeons Museum, and other London institutions, our authors command a wealth of material unobtainable elsewhere, and they are thus enabled not merely to record facts of adult anatomy, but, by comparing individuals of species, to approximately determine the limitations of individual variation. Accurate observations, when systematically arranged, have a permanent value in the progress of science; and, confessing to an admiration of the laborious persistence with which our authors have persevered in their task, we sincerely hope they will continue it until each and every order of Mammals shall have been investigated.

REV. BARTHOLOMEW PRICE, F.R.S.

THE death of the Rev. Bartholomew Price, F.R.S., on Thursday last, deprives the University of Oxford of one who worked long and loyally for its welfare. He took a most active part in the business of the University and of his College, and on many occasions helped to further the interests of science at Oxford. For these labours and for his attractive personal qualities he will be long remembered by many old pupils and friends.

Dr. Price was born in 1818 at Coln St. Dennis, Gloucestershire, and was educated privately at Pembroke College, whence he obtained a first class in mathematics in 1840. He gained the University Mathematical Scholarship in 1842, and two years later was elected Fellow of his College. In 1844 he became tutor, and nine years afterwards Sedleian Professor of Natural Philosophy. In 1852 appeared the first volume of his elaborate work on the infinitesimal calculus, dealing with the differential calculus; the second, on the integral calculus and calculus of variations, was published in 1854; the third, on statics and dynamics of a particle, appeared in 1856; and the last of the four, on the dynamics of material systems, was published in 1862. This treatise obtained for him a considerable reputation in the mathematical world; but his principal work in life was practical, and he will be remembered rather as the active secretary of the University Press during the years of its first great activities after the death of Dean Gaisford, than as a Mathematical Professor. Prof. Price resigned the secretaryship of the Clarendon Press in 1885, when he was succeeded by Mr. P. Lyttelton Gell, who held the post till a few months ago.

Dr. Price was appointed Sedleian professor of natural philosophy at Oxford in 1853, and he only retired from his post in June last, upon attaining his eightieth year. The event was commemorated by a dinner, at which numerous old pupils and others showed the esteem in which they held their counsellor and friend. For many years, both before and after his appointment to the chair of Natural Philosophy, the greater part of the mathematical teaching of the University was in his hands. He was one of the public examiners in mathematical and physical sciences eleven times in twenty-four years, and his works on the differential and integral calculus, &c., were for long the recognised text-books.

Dr. Price was elected a Fellow of the Royal Society in 1852, and he served on the Council of the Society no less than five times. He also served on the Royal Com-

[1] Prof. Righi's elegant experiment was brought before the notice of the British Association in September last by Prof. S. P. Thompson, and three or four days afterwards, with kind permission, I made the observations here described in Prof. Barrett's laboratory in the Royal College of Science, Dublin.

mission to inquire into property and incomes of the Universities of Oxford and Cambridge. He was a Curator of the Bodleian Library, an honorary Fellow of Queen's College, a member of the governing body of Winchester College, and a visitor of the Greenwich Observatory. He was appointed Master of Pembroke College in 1891.

By his remarkable diligence and geniality, and his services in furthering scientific interests at Oxford, Prof. Price became a distinguished feature of his University, where his death will be deeply regretted.

NOTES.

SCIENCE is to be congratulated that two well-known workers for its advancement are included in the list of New Year honours. We refer to Prof. W. C. Roberts-Austen, who has been promoted from the rank of Companion of the Order of the Bath (C.B.) to a Knight Commandership (K.C.B.), and Mr. W. T. Thiselton-Dyer, who has been raised to the rank of Knight Commander of the Order of St. Michael and St. George (K.C.M.G.). Two distinguished members of the medical profession have had honours conferred upon them—Sir Henry Thompson receiving the dignity of a baronetcy, and Dr. Hermann Weber the honour of knighthood. Mr. J. F. Flannery, ex-President of the Institute of Marine Engineers, has also been knighted.

THE recent retirement of Sir John Evans from the Treasurership of the Royal Society, after a period of service of twenty years, has given an opportunity for Fellows of the Society to show their appreciation of the efficient manner in which he discharged the duties of his office. It is proposed to have his portrait painted in oil colours, and to hang it on the walls of the Society's apartments at Burlington House. This would be an appropriate form of recognition of the long association of the Treasurer with the interests of the Society. A number of subscriptions have already been received, and any Fellows of the Society who wish to contribute should send their donations to the Assistant Secretary.

A TELEGRAM from Sydney has been received at the Royal Society stating that the boring into the coral at Funafuti had been discontinued on reaching a depth of 1114 feet. An account of recent operations at Funafuti appeared in NATURE of November 3, 1898 (p. 22).

AT St. Petersburg, last week, a Pan-Russian congress of climatology, hydrology, and balneology was opened by the Grand Duke Paul, as president of the Society for the Preservation of Public Health, for the discussion of means of improving and better utilising the health resorts, bathing places, and mineral waters which now abound in various parts of the empire, especially in the Crimea and the Caucasus.

THE French Government is about to adopt precautionary measures against the introduction of the San José scale into France. Decrees will be issued prohibiting the importation of trees, shrubs and plants from the United States, and requiring an inspection of all fruits, fresh and dried, at the point of landing in France.

THE death of Dr. John Stillwell Schanck, emeritus professor of chemistry and hygiene in Princeton University, is recorded in *Science*. Dr. Schanck was born in 1817, and began the practice of medicine at Princeton in 1843. In 1847 he was made lecturer in zoology at the College, and in 1856 was elected professor of chemistry, to which the chair of natural history was added in 1869. In 1874 the professorship was limited to chemistry, and from 1885 until he was made emeritus professor, in 1892, his chair was entitled chemistry and hygiene.

M. MAURICE LÉVY has been elected Vice-President of the Paris Academy of Sciences for the current year, in succession to M. van Tieghem, who passes to the presidential fauteuil.

IN the report of the meeting of the Kent Coal Company (*Standard*, December 24), the Chairman is stated to have said that "if there was coal in Kent it must affect that Company for good. If there was not, then every scientific man had been wrong. He did not think that could be." The Dover boring has certainly proved the presence of coal-measures in Kent, but no scientific man can say how far these underground coal-measures extend.

THE *Electrical Review* announces that a competition has just been opened by the Swiss Society of Chemical Industry for a treatise which shall be of service in aiding the development of the electro-chemical industry in Switzerland. A premium of 2000 francs (80*l.*) is being offered to the author of the work which shall be adjudged the best of those sent in. All competitors must send in their work by May 1, 1900, to Herr Dr. Henry Schaeppi, president, Die Schweizerische Gesellschaft für Chemische Industrie, Mitlödi, Switzerland, from whom full particulars of the competition may be obtained.

IT is announced by the *British Medical Journal* that an international congress on tuberculosis and the methods for combating it will be held in Berlin from May 23 to 27. The Imperial Chancellor, Prince Hohenlohe, will preside, and will be supported by an influential Committee, headed by the Duke of Ratibon and Prof. von Leyden. Five divisions of the subject have been agreed on: (1) propagation, (2) etiology, (3) prophylaxis, (4) therapeutics, (5) sanatoria. Each of these questions will be introduced by a short and concise address, so as to leave ample time for free discussion and debate.

THE twenty-sixth annual dinner of old students of the Royal School of Mines will take place at the Hotel Cecil on Friday, January 27. The chair will be taken by Mr. F. W. Harbord. Tickets may be obtained from Mr. H. G. Graves, 5 Robert Street, Adelphi, London, W.C.

MR. G. E. LUMSDEN informs us that he has been asked by the President of the Astronomical and Physical Society of Toronto to collect such confirmatory data as are accessible in respect of the contention that associated with certain lightning-flashes and displays of aurora, there are black or dark phenomena. He would be glad to obtain evidence upon this subject, and would be prepared to purchase, if necessary, photographs or lantern slides of lightning flashes, where black streaks or other phenomena are undoubtedly present. His address is The Parliament Buildings, Toronto, Canada.

SOME interesting facts with reference to the mining and minting of gold and silver are mentioned by Mr. Alexander E. Outerbridge, jun., in a paper published in the December *Journal* of the Franklin Institute. It is remarked that notwithstanding the excitement over the Klondike discoveries, the output from that region is insignificant when compared with the total product of the whole country. Statistics show that the State of Colorado alone added twice as much gold to the world's stock in 1897 as did the Klondike region, and several other States largely exceeded the Klondike output. As to the South African gold fields, American mining engineers who have been engaged in developing them, have computed from surveys of the ground and numerous assays of samples of ore taken from different localities and at different depths, that the gold contained in the ore in sight in the district known as the "Rand," or Witwaatersrand, amounts to the enormous value approximately of 800,000,000*l.*, and at the present rate of production it will take half a century to exhaust these deposits, even though no further discoveries be made. Australasia and the

United States together produced more than one-half of the entire output of gold in 1897, and it is in these two countries that modern methods have been most largely adopted.

THE Decimal Association has taken a leading part in educating public opinion, and in making known the advantages of the metric system. The system is used so extensively in scientific work that it is unnecessary to explain in these columns its claims for adoption. The strongest argument in favour of metric weights and measures is the fact that nearly all the leading civilised countries of the world, as well as nearly all our competitors in foreign trade, use them exclusively. From a table given in a pamphlet published by the Decimal Association, it appears that the countries which have already adopted the metric weights and measures represent a population of over 448 millions. Our consuls have almost unanimously reported in favour of the system; our chief Chambers of Commerce have passed resolutions in favour of it; in the new "British Pharmacopœia" the use of the system has been much extended; and numerous trade associations have given support to it; while in no single case has any body of wholesale or retail traders opposed its compulsory adoption. It is now especially urgent that an Act should be passed to make the change compulsory, and all who are interested in this important question are urged to become members of the Decimal Association, and to give it the benefit of their influence and co-operation. The Secretary is Mr. Edward Johnson, Botolph House, Eastcheap, London, E.C.

REFERENCE has been made in these columns on several occasions to Prof. Grassi's work in tracing the propagation of malarial germs by mosquitoes. Further information is now brought to light by communications read before the Reale Accademia dei Lincei during the past month, and published in their *Atti*, vii. 9 and 11. Messrs. G. Bastianelli, A. Bignami and Grassi have now succeeded in tracing the development of the semilunar bodies in the medial intestine of *Anopheles claviger*, kept at a suitable temperature, after these insects had drawn blood from individuals affected by æstivo-autumnal malaria. They have also obtained spring fever by the sole agency of punctures of *A. claviger* in an individual not previously afflicted by malaria and living in a non-malarial locality. Most recently they have observed the later stages of the semilunar bodies in *A. claviger*, and have obtained capsules containing an enormous quantity of filiform bodies apparently non-motile, with one or two amorphous residual masses. The life-history of the malarial parasite is thus proved to be an authenticated case of heterœcism. Concurrently with these observations, Dr. Antonio Dionisi has studied the life-cycle of the endoglobular parasite of the bat, which appears to undergo a similar alternation of generations, the intermediary host being an insect. These conclusions agree with Ross's investigations on the malarial parasites of birds.

THE discussion of the meteorological observations recorded at Trevandrum, under the superintendence of Mr. J. Allan Broun, during the years 1853 to 1864, is published in vol. x. part i. of the *Indian Meteorological Memoirs*. The observatory, which was established by the Maharajah of Travancore, was situated approximately in latitude 8° 31′ N., longitude 76° 59′ E. The observations were deposited at the Meteorological Office in London, and the Meteorological Council, recognising the high scientific value of the data, requested the Secretary of State for India, through the Royal Society, to undertake their discussion, which was agreed to, and the work has been ably carried out under the supervision of Mr. J. Eliot, Meteorological Reporter to the Government of India. We can only very briefly refer to a few of the valuable results obtained. The highest temperature observed was 94°·6, in March, and the lowest 59°·9, in December, giving a total range of 34°·7. The highest and lowest mean daily temperatures were 84°·5 and 70°·9, and the highest and lowest mean monthly temperatures were 82°·9 and 75° respectively. The greatest change in twenty-four hours was 5°·1, in May. One of the most noteworthy features of the meteorology of Trevandrum is the remarkable steadiness of barometric pressure; it is highest from January 11–15 (29·731″), it decreases slowly, but steadily, until the end of May, when it is 29·621″, and it increases during the remainder of the year. The absolute maximum of the diurnal range is ·132″, in February, and the absolute minimum ·099″, in July. The relative humidity is large in all months, varying from a minimum of 72·6, in February, to 86·8, in July. The annual rainfall varies considerably; the mean for thirty-nine years (1838 to 1876) is 64·4″. The mean for the three years of greatest fall was 27·7″ more, and the mean for the three years of least fall was 25·8″ less than the mean fall.

WE have received a valuable quarto memoir by P. José Coronas, of the Manila Observatory, on the eruption of the volcano Mayon, which occurred on June 25–26, 1897. Mayon is the largest volcano in the island of Luzon, and a brief account is given of the principal known eruptions prior to 1897; but, with three exceptions (those of 1616, 1766 and 1800), these all belong to the present century. In a series of interesting chapters, Father Coronas describes the preparatory phenomena (including the earthquake of May 13), the beginning of the eruption, the ejection of lava, the rain of ashes and dust (which extended to about one hundred miles east of the volcano), the detonations and other subterranean noises, the storm on the volcano, and the end of the eruption. The memoir is issued from the private press of the observatory, and is illustrated by four plates and three figures in the text.

THE native arithmetic of Murray Islands, Torres Strait, is described by the Rev. A. E. Hunt in the latest *Journal* (New Series, vol. i. Nos. 1 and 2) of the Anthropological Institute. The only native numerals are netat (one) and neis (two). Higher numbers would be described either by reduplication, as neis netat, literally, two-one for three; neis-i-neis, or two-two for four, &c., or by reference to some part of the body. By the latter method a total of thirty-one could be counted. The counting commenced at the little finger of the left hand, thence counting the digits, wrist, elbow, armpit, shoulder, hollow above the clavicle, thorax, and thence in reverse order down the right arm, ending with little finger of right hand. This gives twenty-one. The toes are then resorted to, and these give ten more. Beyond this number the term gaire (many) would be used. English numerals are now in general use in the Islands.

THE Chinch Bug (*Blissus leucopterus*, Say), one of the most destructive of North American insects, forms the subject of *Bulletin* No. 15 (New Series) of the U.S. Department of Agriculture, Division of Entomology. A previous report on the same subject, by Dr. L. O. Howard, was published as No. 17 of the Old Series of the *Bulletin*; the present has been prepared and brought up to date by Prof. F. M. Webster, the entomologist of the Ohio Agricultural Experiment Station, under Dr. Howard's supervision. The Report deals with the probable origin, diffusion and habits of the insect, with notices of the natural checks to its increase, and the remedial and preventive measures which may be used against it. The Report also includes a notice of various American *Hemiptera* which are likely to be mistaken for the Chinch Bug; and of an allied species (*Blissus doriae*, Ferr.) which occcurs in Italy and Hungary.

DR. G. CHRISTIAN HOFFMANN sends a report of the Section of Chemistry and Mineralogy of the Geological Survey of Canada. Aided by two assistants, Mr. F. G. Wait and Mr.

R. A. A. Johnston, analyses have been made of coals and lignites, of various minerals, ores, rocks, and waters. Among the minerals, there is one named Baddeckite, a new variety of Muscovite, found at Baddeck, Victoria Co., Nova Scotia. It is a hydro-muscovite, in which a very large proportion of the alumina is replaced by ferric oxide. The occurrence in Canada of Chalcanthite and Xenotime are for the first time recorded. An analysis of a spring-water on Cañon Creek, in Alaska, proved the presence of 266 grains of mineral matter—chiefly salts of lime and magnesia.

THE Devonian fossils of Canada have for many years been a subject of special study by Mr. J. F. Whiteaves, palæontologist to the Geological Survey of Canada. In the concluding part of the first volume of "Contributions to Canadian Palæontology," Mr. Whiteaves describes some additional or imperfectly understood fossils from the Hamilton formation of Ontario. These include some supposed Cliona-borings, Corals, Crinoids, Polyzoa, Mollusca, Crustacea and Fishes. Mr. Whiteaves adds a revised list of all the known fossils from the Hamilton formation. Of these, *Atrypa reticularis* appears to be the only well-known European species. The Fishes include *Ptyctodus* and *Aspidichthys*.

MR. JOHN H. SCHAFFNER, of the Ohio State University, writing in the *Journal of Applied Microscopy*, recommends the use of anilin safranin and gentian violet as affording a very good and durable stain for starch grains.

FROM Prof. Jamshedji Edalji we have received papers on "Reciprocal Polygons" and "Associated Conics," in which many well-known theorems and other less-known properties of conics are proved, the method of reciprocation being largely used. The properties of ellipses touching a pair of conjugate hyperbolas afford interesting exercises, but we could wish that the text and figures were rather better printed.

MESSRS. G. W. BACON AND CO., LTD., have just published a well-coloured wall diagram of common poisonous plants. The pictures of the foxglove, woody nightshade, and monkshood are particularly good. The chart should be very useful in country schools to familiarise children with the plants to be avoided.

PROF. MANDEL'S translation of Prof. Hammarsten's "Textbook of Physiological Chemistry" was reviewed in these columns four years ago (vol. l. p. 449). A second American edition, prepared from the third revised German edition by the same translator, who produced an excellent English version of the earlier one, has now been published by Messrs. John Wiley and Sons. The new volume is essentially the same as the first edition, being simply brought up to date.

A COPY of the first number of *Science Work*—a monthly review of scientific literature—has been received. The contents consist of a few general notes, the titles of a selection of articles which have recently appeared in some periodicals, a directory of lecturers, and a synopsis of some scientific expeditions now in the field. The publishers of this venture are Messrs. Robert Aikman and Co., Manchester.

WE have received a copy of a pamphlet entitled "Replica di Krupp alla Protesta del Signor Bashford," published by the Cambridge University Press. In the *Revista di Artiglierca e Genio*, Dr. Bashforth charged the Krupp firm with making use of his general tables of velocity and time, and velocity and space, containing the results of his experiments, without acknowledgment. Mr. F. Krupp has replied on behalf of the firm, and the pamphlet now received contains a translation of this letter presenting the Krupp view of the matter, with an introduction and notes by Dr. Bashforth, which he holds afford ample justification for his protest.

THE last two parts received (Heft 3 and 4) of Engler's *Botanische Jahrbücher für Systematik, Pflanzengeschichte, u. Pflanzengeographie* contain the following articles:—The conclusion of Hieronymus's account of the Pteridophyta of Argentina; Anderson on the History of the Vegetation of Sweden; Höck on the Flora of the alder-plantations of North Germany; Fedtsckenko on the Flora of the southern Altai mountains; a continuation of Pfitzer's Classification of the Orchideæ; the commencement of a paper by Perkins on the Monimiaceæ; and a description by Hallier of *Hildebrandtia*, a new diœcious genus of Convolvulaceæ.

THE number of new syntheses capable of being carried out with the aid of diazomethane, would appear to be by no means exhausted. Prof. H. von Pechmann describes in the current number of the *Berichte* a new synthesis of pyrazol by means of this active reagent. A solution of diazomethane in ether is cooled to 0° C. and saturated with dry acetylene in a large flask, and the whole allowed to stand. The reaction is rather slow, about two days being required; simple evaporation then deposits pure pyrazol in fine prisms in quantity about fifty per cent. of the theoretical value.

THE announcement is made (by M. and Mme. Curie and M. Bémont), in the current number of the *Comptes rendus*, of the probable existence of a new element, to which the name of radium is provisionally given. It will be remembered that M. Becquerel discovered that the metal uranium and its salts possessed the remarkable property of emitting rays much resembling the X-rays in penetrative power and action upon electrified gases. Using this new weapon of research, M. and Mme. Curie were able to isolate from pitchblende a new element (*polonium*) resembling zinc in its chemical properties, differing from this metal, however, in possessing radio-active properties of a similar character to uranium, but of greater intensity. In following up this work they (in conjunction with M. Bémont) have now obtained indications of another element possessing still more powerful radio-active properties, and chemically resembling barium, inasmuch as it is precipitated neither by sulphuretted hydrogen, by ammonium sulphide, nor by ammonia; the sulphate is insoluble in water and in acids; the carbonate insoluble in water; and the chloride, although very soluble in water, insoluble in alcohol. Fractional precipitation of the chlorides by alcohol showed an increasing amount of radium in the precipitate, as measured by its radio-activity, the final fractions possessing an activity 900 times greater than uranium. The spectrum of this chloride was found by M. Demarcay to give, besides the lines of barium, lead, calcium and platinum, a strong new line ($\lambda = 3814{\cdot}8$), which in his opinion confirms the existence of a new element.

IN a recent number of the *Proceedings* of the American Academy of Arts and Sciences, Prof. T. W. Richards, of Harvard, describes a series of interesting experiments on the retention and release of gases occluded by the oxides of metals, a subject of special importance to those engaged in the determination of atomic weights. He shows that oxide of copper prepared by ignition of the nitrate contains between four and five times its volume of occluded gases up to a temperature of 860° C. Slightly above this temperature the bulk of the gas is given off, but before 1000° is reached the copper oxide itself decomposes. Of the gases resulting from the decomposition of the nitrate, the oxygen escapes more rapidly than the nitrogen. The explanation given by Prof. Richards is that the gases proceed from basic nitrate imprisoned in the oxide, the N_2O_5 being resolved into nitrogen and oxygen. The oxygen escapes more rapidly than the nitrogen, by uniting with metal or a lower oxide which has momentarily released some of its proper supply of oxygen. The released oxygen passes onwards and

outwards by a similar process, so that there is a continuous transference of oxygen by a sort of Grotthus chain of unstable copper oxide molecules. This view is in harmony with the fact that cupric oxide, the most easily reducible of the oxides investigated, is the one which parts with its occluded oxygen most easily, at the same time obstinately retaining the nitrogen, whilst the oxides of zinc and magnesium, being more stable, retain the oxygen persistently. It was found that the electrical conductivity of copper oxide was much increased at a temperature approaching redness, whilst that of zinc oxide was much less increased, and that of magnesium oxide not at all. This would be explained by the copper oxide being to a certain extent dissociated by heat, and permitting of some metallic conduction. Cupric oxide was found to give up oxygen steadily when heated in vacuo to 790°, cuprous oxide being found in the residue.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*, ♀) from India, presented by Mr. A. Urban Smith; six Snow Buntings (*Plectrophanax nivalis*), European, purchased; three Grey Squirrels (*Sciurus cinereus*) from North America, deposited; a Great Eagle Owl (*Bubo maximus*), bred in the Gardens.

Errata.—In the necrology of Prof. Allman, p. 202, line 18, delete "Regius"; p. 204, line 37, for "Grumera" read "Gunnera."

OUR ASTRONOMICAL COLUMN.

COMET CHASE.—The following ephemeris of Comet Chase is a continuation of that which we have previously published from Herr J. Möller's computation:—

1899.	R.A. (app.) h. m. s.	Decl (app.) ° ′	Br.
Jan. 1	11 5 50	+29 23·5	1
7	6 45	29 46·1	1
9	7 33	30 8·7	1
11	8 14	30 31·5	1
13	8 48	30 54·6	1
15	11 9 15	+31 17·9	1

PLANET WITT (DQ 1898).—Although only of the twelfth magnitude, planet Witt should be closely followed and good positions measured as opportunities afford. The accompanying ephemeris for the coming fortnight will prove useful to those whose instruments are large enough to follow this interesting body.

Berlin Midnight.

1898.	R.A. h. m. s.	Dec. ° ′
Jan. 5	23 3 0	+4 37·6
7	7 51	5 3·3
9	12 45	5 29·6
11	17 42	5 56·1
13	22 44	6 23·0
15	27 48	6 50·2
17	32 56	7 17·7
19	23 38 6	+7 45·4

The above ephemeris, calculated by Dr. E. Millosevich (*Astr. Nachr.*, 3532), requires, according to an observation by Dr. Cerulli, a correction of about − 5s. in R.A., and − 0′·3 in declination.

ASTRONOMICAL PHOTOGRAPHY WITH SMALL INSTRUMENTS.—The earnest worker, even with only small means at his disposal, may perform important work in astronomy by the aid of lenses of comparatively small aperture. The great advances made in the sensitiveness of dry plates helps us now to photograph quite easily objects which formerly presented a great difficulty.

By replacing the eyepiece of a good telescope with a small camera, with a suitable positive or negative lens, very useful work can be done, although the scale of magnification need not be very great. The employment of a negative lens makes the instrument more convenient for use, as a shorter telescope tube can be utilised.

One great function of photographs of celestial objects is to record changes in the appearance of bodies or regions studied. In the case of the former large images are required, and the phototelescopes are the most suitable; in the latter, large fields, but not necessarily very large magnification, are required for many purposes of great interest. In the *Photographische Correspondenz*, Herr P. Zschokke points out in an interesting article the useful work an amateur can accomplish by means of small instruments, and gives an interesting table showing the relative sizes of apertures, focal lengths of objectives, focal lengths of combinations, &c. Since, in this class of instrument, the size of the diameter of the image formed is the main point, we give below the table containing these values, which has been reproduced in the *British Journal of Photography* (December 23, 1898). All the figures in the following columns refer to millimetres.

Aperture.	Focus of objective.	Focus of tele-photo combination.	Entire length of telescope.	Diameter of image of sun or moon.
61	730	5370	980	50
81	1218	8060	1350	75
108	1624	11370	1960	108
135	1949	16115	2365	150
135	1353	16115	1800	150
162	2599	32230	3510	300
162	2599	24170	3155	225

A NEW VARIABLE IN CASSIOPEIA.—Dr. T. D. Anderson, writing in the *Astr. Nachr.* (No. 3533), states that the star which has the position for 1855, R.A. 23h. 37·5m., and Decl. + 55° 45′, is a hitherto unnoticed variable. This star, which is not recorded in the BD, was first seen by Dr. Anderson in the beginning of 1897, and was then only 0·2 magnitudes fainter than + 55°·3007, the magnitude of which was then fairly estimated as 9·0. In September 23 of the same year the star was found missing, although another star of magnitude 10·3 could be easily seen 3′ to the north of the missing star. Subsequent searches failed to pick up the same star again; and Dr. Anderson, coming to the conclusion that the object was probably a Nova, only examined the region very occasionally.

Last month, however, on the 5th, he again examined the region, and at last found the object of his search. At this time it excelled in brilliancy its neighbour of magnitude 10·3 by as much as it was itself surpassed by BD + 55°·3007.

OBSERVATIONS OF α ORIONIS.—Mr. R. T. A. Innes, writing from the Cape Observatory (*Astr. Nachr.*, 3533), mentions that he has been observing α Orionis (Betelgeuze) on several evenings, and the star was found to be only slightly brighter than Aldebaran. Last season, as Mr. Innes states, Betelgeuze was twice as bright as Aldebaran, which is its ordinary state. From this observation it may be concluded that one of the irregular minima of Betelgeuze is approaching.

PRIZES PROPOSED BY THE PARIS ACADEMY OF SCIENCES FOR 1899.

THE following are the prize subjects proposed by the French Academy of Sciences for the present year.

The Bordin Prize (3000 fr.) for a study of the questions relating to the determination, properties, and application of systems of orthogonal curvilinear coordinates of *n* variables, indicating particularly, in as precise a manner as possible, the degree of generality of these systems; the Francœur Prize (1000 fr.) and the Poncelet Prize (2000 fr.), for useful work in the field of pure and applied mathematics. In Mechanics is offered the Extraordinary Prize of 6000 francs, for any invention increasing the efficiency of the French naval forces; the Montyon Prize (700 fr.), for the invention or improvement of instruments useful to the progress of agriculture or the mechanical arts; the Plumey Prize (2500 fr.), for any invention contributing to the progress of steam navigation; the Tourneyron Prize, for an improvement in any point of the theory of pumps. In Astronomy, the Lalande Prize (540 fr.), for the most interesting or useful astronomical observation made during the year: the Valz Prize (460 fr.) for astronomical work. In Physics, the La Caze Prize (10,000 fr.). In Statistics, the Montyon Prize (500 fr.). In Chemistry, the Jecker Prize (10,000 fr.) for discoveries in organic chemistry; the Cahours Prize (3000 fr.) for the encouragement of research in young chemists already known by their published work; and a La Caze Prize of 10,000 fr. In Mineralogy and Geology, the Delesse Prize (1400 fr.), and the

Fontannes Prize (2000 fr.) for researches in palæontology. In Biology, the *Grand Prix* of the Physical Sciences (3000 fr.) for a study of the biology of Nematods, especially the forms and conditions of their reproduction, and the Bordin Prize (3000 fr.). In Botany, the Desmazières Prize (1600 fr.) for work on the Cryptogams; the Montagne Prizes (1000 fr. and 500 fr.) for important work bearing on the anatomy, physiology, development, or description of the lower Cryptogams; and the Thore Prize (200 fr.) for the best memoir on the cellular Cryptogams of Europe. In Anatomy and Zoology, the Savigny Prize (975 fr.) for the assistance of young travelling zoologists. In Medicine and Surgery, a Montyon Prize for discoveries or inventions having a direct bearing upon the art of healing; the Barbier Prize (2000 fr.) for similar objects; the Bréant Prize (100,000 fr.) for the discovery of an absolute specific against Asiatic cholera; the Godard Prize (1000 fr.) for the best memoir on the anatomy, physiology, and pathology of the genito-urinary organs; the Serres Prize for a memoir on general embryology applied as far as possible to Physiology and Medicine; the Chaussier Prize (10,000 fr.) for the best book or memoir which has appeared during the last four years, either in medicine, legal or practical medicine; the Bellion Prize (1400 fr.); the Mège Prize (300 fr.); the Lallemand Prize (1800 fr.) for work on the nervous system; and the Baron Larrey Prize (1000 fr.) for a memoir on military or naval medicine, surgery, or hygiene. In Physiology, a Montyon Prize (750 fr.); a La Caze Prize (10,000 fr.); the Pourat Prize (1400 fr.) for a memoir on the specific characters of the contraction of different muscles; and the Philipeaux Prize (890 fr.) for work in experimental physiology. In Physical Geography, the Gay Prize (2500 fr.) for a study of the Mediterranean mollusca, and a comparison of these with those found on the French oceanic coasts.

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Of these the prizes bearing the names of Wilde, Tchihatchef, Leconte, Desmazières, Delesse, La Caze, and Lalande are expressly stated to be offered without distinction of nationality.

MAGNETIC SURVEYS.[1]

THE importance of magnetic survey work has been recognised for a long time. Many of the most eminent scientific men of the century now about to close have devoted much time and thought to magnetic observation and the reduction of results, and their labours have not been without fruit. To them we owe all that is known with certainty as to the magnetic state of the earth and its changes; and though observations have no doubt to some degree outpaced the work of reduction and the construction of theory, much has been done to construct from observational data a general theory of terrestrial magnetism. We have the great mathematical theory of Gauss based on the results of Sabine, Barlow, Horner, and others, with the answer it gives to the question of the locality of the magnetic distribution which gives rise to the ordinary phenomena of terrestrial magnetism. Also, and founded to some extent on Gauss's theory, we have now that of Schuster on the daily changes of the magnetic forces and their causes, which has yielded most important results, as to the locality of the sources of this periodic disturbance. As a consequence of Schuster's theory there have lately been published some important discussions of the diurnal changes and their theory by von Bezold and others.

The secular changes have received a great accession of interest since Bauer's first discussion of the subject some years ago in an inaugural dissertation at the University of Berlin. In that he gave the method of graphical representation of the secular changes now known by his name. An observer is supposed to look from the centre of a magnet, freely suspended at its centre of gravity, and to note the curve which the north-pointing pole seems to describe, and which shows changes of dip as well as changes of magnetic declination. This mode of representation can, of course, be applied to the diurnal changes, and shows that though a complete cycle of secular changes is not yet available from observations, yet the diagram of the diurnal changes may serve to complete it. Let the secular changes be produced by the combination with a constant magnetic system rotating with the earth of a second system, also turning with the earth, but while doing so describing a secular orbit round the earth's axis. This is the hypothesis of Wilde. Now let a needle be imagined suspended outside the earth while the earth turns beneath it. The superimposed magnetic system will in the course of one rotation of the earth occupy the same position relatively to the suspended needle that it successively occupies in the secular period with reference to a needle fixed with the earth and turning with it. The curve thus obtained for a single day will, if the second system be invariable and its successive positions be symmetrical about the earth's axis, be the same as the secular curve. Though this curve cannot be directly observed, for no needle can be suspended in the manner supposed, the distribution of magnetic force round the earth is sufficiently well known to enable the positions of the needle to be calculated and the curve laid down on paper. The indications at the same instant of time of needles suspended at different points of the same astronomical circle of latitude, in fact, give the curve. There is sufficient similarity between the curves obtained in this way[1] to afford a fairly satisfactory first test of Wilde's hypothesis, and no doubt further progress in the solution of this interesting question will shortly be obtained by Dr. Bauer himself, or others.

The origin of the diurnal and secular variations may perhaps become known fully only when the secret of terrestrial magnetism itself is revealed. In the meantime there can be no question of the absorbing interest of the problem, and of the fact that the only way to solve it is by means of continued magnetic observation at different parts of the world, at sufficiently equipped and properly chosen magnetic observatories. It is to be hoped that as the value of observations, so far as the secular changes are concerned, depends on their comparison with those obtained much later in time, care will be taken in future by municipal and government authorities not to injure or render nugatory the work done by bringing disturbing electric currents and traffic into the vicinity of observatories. The fact that a sub-section (of Section A) of the British Association gave itself at the recent meeting to the discussion of terrestrial magnetic problems is at once an indication of fuller recognition of the interesting nature and the importance of magnetic research, and a guarantee that magnetic observatories and laboratories will be made to co-operate to the best advantage in the work to be done, and that everything will be done to induce authorities to protect them when powers are being sought for engineering projects.

Dr. Bauer has set before us a brief but interesting statement of the history of magnetic research, the objects to be served by it, the kind of survey work that may be attempted best by observers in different localities, and the mode in which it should be carried out, illustrated by an account of a survey of part of the State of Maryland which he has lately undertaken.

There are many notes of great interest in this historical sketch. For example we find, what we confess we did not know before, that the discoverer of the declination of the compass needle from the true north was Christopher Columbus. It seems that on September 13, 1492, Columbus crossed the agonic line, the line of no declination, a little to the west of the Island of Fayal in the Azores, and observed that the compass at places on the east of this line pointed east, and at places on the west pointed west of the true north. This line ran, curiously enough, along the old boundary between the kingdoms of Portugal and Castile. Generally Columbus has received credit only for discovering the agonic line, owing to a statement of Formaleoni, the Italian historian, that Bianco's chart of 1436 contained values of the declination, an error which Humboldt perpetuated by repeating in his "Cosmos." The recorded value of the magnetic declination at Rome in 1269, ascribed to Petrus Peregrinus, was inserted, it appears, by some one early in the sixteenth century, thus subsequent to Columbus.

That the declination had not been discovered sooner arose

[1] "Maryland Geological Survey. First Report upon Magnetic Work in Maryland, including the History and Objects of Magnetic Surveys." By L. A. Bauer. Special Publication, Vol. I, Part V. Pp. v + 125, with one plate. (Baltimore: The Johns Hopkins Press, 1897.)

[1] See Prof. Rücker's Rede Lecture, NATURE, December 23, 1897.

from the fact that it was but small over the greater part of the Mediterranean, and so for long the deep-sea sailors of Europe were without much experience of its effects. When it did force itself on their notice it was supposed to be due to an error of construction, and a correction was made so that the point marked north on the card pointed to the astronomical north.

The importance of Gilbert's "De Magnete" has only been fully seen in the light of modern magnetic research. It constituted in itself an advance which has not been equalled by any single step during the three hundred years that have elapsed since its publication. So far as observation and theory have yet gone, even with the aid of Gauss's refined analysis, they have only confirmed the conclusion there set forth, *Magnus magnes ipse est globus terrestris*—"the earth is a great magnet." It is of great importance to decide whether or not the earth is a permanent magnet like a piece of steel, but the decision that its magnetism is induced by currents of electricity will not in any way contradict Gilbert's assertion.

Magnetic survey work has of course its own practical and commercial importance, but in a country like America, where public and private boundaries have been laid down by the compass, the determination of the secular variation is of peculiar interest. Without it the results of early land surveys could not now be interpreted.

In all cases the earlier surveys in America were compass surveys, and the directions referred to the magnetic meridian. Hence, as the deviation of the magnetic from the true meridian was known only in a few cases, the directions laid down in old topographical maps and plans of estates can only be recovered by determining the deviations now, and applying the known value of the secular variation. And Dr. Bauer says that one of the Eastern states actually still retains the magnetic meridian as the legal meridian of reference for land surveys. We had thought that such conservatism was unknown on the other side of the Atlantic.

Dr. Bauer quotes Robert Norman's own account of his discovery of the dip.[1] It has been quoted before, but not many have seen it, so we venture here to make a short extract. After stating that he had repeatedly found needles which had been finished and balanced before magnetisation inclining themselves to the horizontal with the north pole down when magnetised by being stroked with a lodestone, so that he had to "put some small piece of wire on the south point, and make it equall againe," but had put the result down to defect of construction rather than to "anie such propertie in the stone," he goes on: "It chaunced at length that there came to my hands an instrument to be made with a needle of sixe inches long, which needle after I had polished, cut of a just length, and made it stand levell upon a pin, so that nothing rested but onlie the touching it with the stone; when I had touched the same, presentlie the north part thereof declined down in such sort that being constrained to cut awaie some of that part to make it equall againe, in the end I cut it too short, and so spoiled the needle wherein I had taken so much paines. Hereby being stroken into some cholar, I applied myself to seeke further into this effect. . . ."

The first systematic surveys were made by William Whiston, better known perhaps as the translator of "Josephus." He was active in carrying on magnetic observations with a view to the determination magnetically of geographical position, and first drew isoclinics, made relative intensity observations, and invented the vibration method of obtaining the dip. His main object was to obtain the prize offered for the best solution of the problem of determining the longitude at sea, and he seems to have been successful in enlisting the interest of the Court, and even in obtaining money grants in aid of his work. Although the problem of the longitude was to be quite otherwise solved, Whiston's researches were of very great value. From Whiston to Gauss is a hundred years, but the next great development in terrestrial magnetic science came when Gauss introduced absolute measurement, and thus led observers to express results in units independent of arbitrary constants, so that they were at once available as data for other investigators. As a result the science has gone on vigorously from then till now developing in interest and importance. This, of course, is only one part of the effect of Gauss's work: it has been equally, if not more, productive of progress in the science of pure and applied electricity.

With respect to the conduct of magnetic survey work generally, Dr. Bauer makes some remarks which it would be well if observers would bear in mind. Much time and labour has been practically lost by the observation of only a single element, such as the declination. As he says, fully 75 per cent. of the time and money spent on a survey is spent in *occupying* stations, and the remainder only in observation. Thus it is only a very little further expense comparatively for an observer to observe a little longer, and get with sufficient accuracy values of all three elements. Thus magnetic declination surveys are of little value by themselves, except for the land surveying application, and those which have been carried out in many places could have been rendered many times more valuable to science by a very trifling additional outlay.

The initiation of terrestrial magnetic work by the British Association, the magnetic surveys carried out in this country by the British Association, and described in Sir Edward Sabine's Report for 1861, the more recent elaborate surveys by Thorpe and Rücker, and the work done in other parts of the world, are all appreciatively referred to.

Now that the British Association has again taken up the work in a very special manner, and a magnetic committee has been appointed, we may confidently expect to see a further great development of the subject. The work done by the conference at Bristol, under the presidency of Prof. Rücker, is a valuable instalment. A. GRAY.

[1] From his book, "The New Attractive," &c., 1576.

PROGRESSIVE EDUCATION.[1]

THE publication of the second and third volumes of the series of reports prepared for the Education Department under the supervision of Mr. Michael E. Sadler, the Director of Special Inquiries, again calls the attention of educationists to the value of the work which is being performed by this latest addition to our national machinery for the guidance and co-ordination of educational effort. These reports, with their predecessors, closely resemble in their scope the publications of the U.S. Bureau of Education, and accentuate the fact that we have only lately commenced to do what has been for some time the established custom in the States. Just as the man of science acquaints himself with the work, failures and successes alike, of previous investigators in the field of his own activity, so to do the best possible work, the teacher, who is also an experimenter, should similarly become familiar with what has been accomplished by other teachers in different parts of the world. With these reports to hand, and others which will doubtless follow, there is no reason why the schoolmaster should not, in the future, start the education of his pupils armed with the experience of many successful veterans.

Though every one of the 1389 pages included in these volumes contains something of interest, this article is to be concerned more especially with what there is to be learnt concerning the present state of the teaching of science—whether in this country or abroad. Of the many plans of treatment which suggest themselves, perhaps, as reference to the volumes will be thereby facilitated, it will be best to take the papers containing important allusions to scientific instruction in the order in which they occur in the volumes themselves.

THE LONDON POLYTECHNIC INSTITUTES.

The London Polytechnic has but little resemblance to the institutions to which a similar name is applied in France, Switzerland, and Germany. Each Polytechnic Institute (of which there are at present eleven, with four branches) is an independent organisation which deliberately combines social intercourse, recreation, and instruction. As educational institutions these polytechnics constitute a new and distinct type; they range from the "Upper Standard" day school for boys and girls of thirteen, up to high University instruction and post-graduate research. All kinds and grades of work go on simultaneously. In one room boys of twelve are learning arithmetic, or girls of thirteen are being taught to sew; in another, classes of plumbers or bricklayers, compositors or tailors, are being practically trained in the processes of their respective crafts; close by is the smithy or the fitting shop, crowded with young engineering artisans; in other class-rooms are groups reading Dante or studying economics; and just at hand are well-equipped physical and chemical laboratories where

[1] "Special Reports on Educational Subjects." Vols. ii. and iii. (Issued by the Education Department, 1898.)

Fontannes Prize (2000 fr.) for researches in palæontology. In Biology, the *Grand Prix* of the Physical Sciences (3000 fr.) for a study of the biology of Nematods, especially the forms and conditions of their reproduction, and the Bordin Prize (3000 fr.). In Botany, the Desmazières Prize (1600 fr.) for work on the Cryptogams; the Montagne Prizes (1000 fr. and 500 fr.) for important work bearing on the anatomy, physiology, development, or description of the lower Cryptogams; and the Thore Prize (200 fr.) for the best memoir on the cellular Cryptogams of Europe. In Anatomy and Zoology, the Savigny Prize (975 fr.) for the assistance of young travelling zoologists. In Medicine and Surgery, a Montyon Prize for discoveries or inventions having a direct bearing upon the art of healing; the Barbier Prize (2000 fr.) for similar objects; the Bréant Prize (100,000 fr.) for the discovery of an absolute specific against Asiatic cholera; the Godard Prize (1000 fr.) for the best memoir on the anatomy, physiology, and pathology of the genito-urinary organs; the Serres Prize for a memoir on general embryology applied as far as possible to Physiology and Medicine; the Chaussier Prize (10,000 fr.) for the best book or memoir which has appeared during the last four years, either in medicine, legal or practical medicine; the Bellion Prize (1400 fr.); the Mège Prize (300 fr.); the Lallemand Prize (1800 fr.) for work on the nervous system; and the Baron Larrey Prize (1000 fr.) for a memoir on military or naval medicine, surgery, or hygiene. In Physiology, a Montyon Prize (750 fr.); a La Caze Prize (10,000 fr.); the Pourat Prize (1400 fr.) for a memoir on the specific characters of the contraction of different muscles; and the Philipeaux Prize (890 fr.) for work in experimental physiology. In Physical Geography, the Gay Prize (2500 fr.) for a study of the Mediterranean mollusca, and a comparison of these with those found on the French oceanic coasts.

Other general prizes offered for 1899 include the Arago Medal, the Montyon Prize (unhealthy trades), the Trémont Prize (1100 fr.), the Gegner Prize (4000 fr.), the Petit D'Ormoy Prizes (10,000 fr. each), one for work done in mathematics, and the other in natural science; the Leconte Prize (50,000 fr.), the Tchihatchef Prize (3000 fr.), the Gaston Plante Prize (3000 fr.), the Houllevigue Prize (500 fr.), the Wilde Prize (4000 fr.), the Saintour Prize (3000 fr.), the Kastner-Boursault Prize (2000 fr.), the Jean-Jacques Berger Prize (12,000 fr.), and the Baron Joest Prize (2000 fr.).

Of these the prizes bearing the names of Wilde, Tchihatchef, Leconte, Desmazières, Delesse, La Caze, and Lalande are expressly stated to be offered without distinction of nationality.

MAGNETIC SURVEYS.[1]

THE importance of magnetic survey work has been recognised for a long time. Many of the most eminent scientific men of the century now about to close have devoted much time and thought to magnetic observation and the reduction of results, and their labours have not been without fruit. To them we owe all that is known with certainty as to the magnetic state of the earth and its changes; and though observations have no doubt to some degree outpaced the work of reduction and the construction of theory, much has been done to construct from observational data a general theory of terrestrial magnetism. We have the great mathematical theory of Gauss based on the results of Sabine, Barlow, Horner, and others, with the answer it gives to the question of the locality of the magnetic distribution which gives rise to the ordinary phenomena of terrestrial magnetism. Also, and founded to some extent on Gauss's theory, we have now that of Schuster on the daily changes of the magnetic forces and their causes, which has yielded most important results, as to the locality of the sources of this periodic disturbance. As a consequence of Schuster's theory there have lately been published some important discussions of the diurnal changes and their theory by von Bezold and others.

The secular changes have received a great accession of interest since Bauer's first discussion of the subject some years ago in an inaugural dissertation at the University of Berlin. In that he gave the method of graphical representation of the secular changes now known by his name. An observer is supposed to look from the centre of a magnet, freely suspended at its centre of gravity, and to note the curve which the north-pointing pole seems to describe, and which shows changes of dip as well as changes of magnetic declination. This mode of representation can, of course, be applied to the diurnal changes, and shows that though a complete cycle of secular changes is not yet available from observations, yet the diagram of the diurnal changes may serve to complete it. Let the secular changes be produced by the combination with a constant magnetic system rotating with the earth of a second system, also turning with the earth, but while doing so describing a secular orbit round the earth's axis. This is the hypothesis of Wilde. Now let a needle be imagined suspended outside the earth while the earth turns beneath it. The superimposed magnetic system will in the course of one rotation of the earth occupy the same position relatively to the suspended needle that it successively occupies in the secular period with reference to a needle fixed with the earth and turning with it. The curve thus obtained for a single day will, if the second system be invariable and its successive positions be symmetrical about the earth's axis, be the same as the secular curve. Though this curve cannot be directly observed, for no needle can be suspended in the manner supposed, the distribution of magnetic force round the earth is sufficiently well known to enable the positions of the needle to be calculated and the curve laid down on paper. The indications at the same instant of time of needles suspended at different points of the same astronomical circle of latitude, in fact, give the curve. There is sufficient similarity between the curves obtained in this way[1] to afford a fairly satisfactory first test of Wilde's hypothesis, and no doubt further progress in the solution of this interesting question will shortly be obtained by Dr. Bauer himself, or others.

The origin of the diurnal and secular variations may perhaps become known fully only when the secret of terrestrial magnetism itself is revealed. In the meantime there can be no question of the absorbing interest of the problem, and of the fact that the only way to solve it is by means of continued magnetic observation at different parts of the world, at sufficiently equipped and properly chosen magnetic observatories. It is to be hoped that as the value of observations, so far as the secular changes are concerned, depends on their comparison with those obtained much later in time, care will be taken in future by municipal and government authorities not to injure or render nugatory the work done by bringing disturbing electric currents and traffic into the vicinity of observatories. The fact that a sub-section (of Section A) of the British Association gave itself at the recent meeting to the discussion of terrestrial magnetic problems is at once an indication of fuller recognition of the interesting nature and the importance of magnetic research, and a guarantee that magnetic observatories and laboratories will be made to co-operate to the best advantage in the work to be done, and that everything will be done to induce authorities to protect them when powers are being sought for engineering projects.

Dr. Bauer has set before us a brief but interesting statement of the history of magnetic research, the objects to be served by it, the kind of survey work that may be attempted best by observers in different localities, and the mode in which it should be carried out, illustrated by an account of a survey of part of the State of Maryland which he has lately undertaken.

There are many notes of great interest in this historical sketch. For example we find, what we confess we did not know before, that the discoverer of the declination of the compass needle from the true north was Christopher Columbus. It seems that on September 13, 1492, Columbus crossed the agonic line, the line of no declination, a little to the west of the Island of Fayal in the Azores, and observed that the compass at places on the east of this line pointed east, and at places on the west pointed west of the true north. This line ran, curiously enough, along the old boundary between the kingdoms of Portugal and Castile. Generally Columbus has received credit only for discovering the agonic line, owing to a statement of Formaleoni, the Italian historian, that Bianco's chart of 1436 contained values of the declination, an error which Humboldt perpetuated by repeating in his "Cosmos." The recorded value of the magnetic declination at Rome in 1269, ascribed to Petrus Peregrinus, was inserted, it appears, by some one early in the sixteenth century, thus subsequent to Columbus.

That the declination had not been discovered sooner arose

[1] "Maryland Geological Survey. First Report upon Magnetic Work in Maryland, including the History and Objects of Magnetic Surveys." By L. A. Bauer. Special Publication, Vol. I, Part V. Pp. v + 125, with one plate. (Baltimore: The Johns Hopkins Press, 1897.)

[1] See Prof. Rücker's Rede Lecture, NATURE, December 23, 1897.

from the fact that it was but small over the greater part of the Mediterranean, and so for long the deep-sea sailors of Europe were without much experience of its effects. When it did force itself on their notice it was supposed to be due to an error of construction, and a correction was made so that the point marked north on the card pointed to the astronomical north.

The importance of Gilbert's "De Magnete" has only been fully seen in the light of modern magnetic research. It constituted in itself an advance which has not been equalled by any single step during the three hundred years that have elapsed since its publication. So far as observation and theory have yet gone, even with the aid of Gauss's refined analysis, they have only confirmed the conclusion there set forth, *Magnus magnes ipse est globus terrestris*—"the earth is a great magnet." It is of great importance to decide whether or not the earth is a permanent magnet like a piece of steel, but the decision that its magnetism is induced by currents of electricity will not in any way contradict Gilbert's assertion.

Magnetic survey work has of course its own practical and commercial importance, but in a country like America, where public and private boundaries have been laid down by the compass, the determination of the secular variation is of peculiar interest. Without it the results of early land surveys could not now be interpreted.

In all cases the earlier surveys in America were compass surveys, and the directions referred to the magnetic meridian. Hence, as the deviation of the magnetic from the true meridian was known only in a few cases, the directions laid down in old topographical maps and plans of estates can only be recovered by determining the deviations now, and applying the known value of the secular variation. And Dr. Bauer says that one of the Eastern states actually still retains the magnetic meridian as the legal meridian of reference for land surveys. We had thought that such conservatism was unknown on the other side of the Atlantic.

Dr. Bauer quotes Robert Norman's own account of his discovery of the dip.[1] It has been quoted before, but not many have seen it, so we venture here to make a short extract. After stating that he had repeatedly found needles which had been finished and balanced before magnetisation inclining themselves to the horizontal with the north pole down when magnetised by being stroked with a lodestone, so that he had to "put some small piece of wire on the south point, and make it equall againe," but had put the result down to defect of construction rather than to "anie such propertie in the stone," he goes on: "It chaunced at length that there came to my hands an instrument to be made with a needle of sixe inches long, which needle after I had polished, cut of a just length, and made it stand levell upon a pin, so that nothing rested but onlie the touching it with the stone; when I had touched the same, presentlie the north part thereof declined down in such sort that being constrained to cut awaie some of that part to make it equall againe, in the end I cut it too short, and so spoiled the needle wherein I had taken so much paines. Hereby being stroken into some cholar, I applied myself to seeke further into this effect. . . ."

The first systematic surveys were made by William Whiston, better known perhaps as the translator of "Josephus." He was active in carrying on magnetic observations with a view to the determination magnetically of geographical position, and first drew isoclinics, made relative intensity observations, and invented the vibration method of obtaining the dip. His main object was to obtain the prize offered for the best solution of the problem of determining the longitude at sea, and he seems to have been successful in enlisting the interest of the Court, and even in obtaining money grants in aid of his work. Although the problem of the longitude was to be quite otherwise solved, Whiston's researches were of very great value. From Whiston to Gauss is a hundred years, but the next great development in terrestrial magnetic science came when Gauss introduced absolute measurement, and thus led observers to express results in units independent of arbitrary constants, so that they were at once available as data for other investigators. As a result the science has gone on vigorously from then till now developing in interest and importance. This, of course, is only one part of the effect of Gauss's work: it has been equally, if not more, productive of progress in the science of pure and applied electricity.

With respect to the conduct of magnetic survey work generally, Dr. Bauer makes some remarks which it would be well if observers would bear in mind. Much time and labour has been practically lost by the observation of only a single element, such as the declination. As he says, fully 75 per cent. of the time and money spent on a survey is spent in *occupying* stations, and the remainder only in observation. Thus it is only a very little further expense comparatively for an observer to observe a little longer, and get with sufficient accuracy values of all three elements. Thus magnetic declination surveys are of little value by themselves, except for the land surveying application, and those which have been carried out in many places could have been rendered many times more valuable to science by a very trifling additional outlay.

The initiation of terrestrial magnetic work by the British Association, the magnetic surveys carried out in this country by the British Association, and described in Sir Edward Sabine's Report for 1861, the more recent elaborate surveys by Thorpe and Rücker, and the work done in other parts of the world, are all appreciatively referred to.

Now that the British Association has again taken up the work in a very special manner, and a magnetic committee has been appointed, we may confidently expect to see a further great development of the subject. The work done by the conference at Bristol, under the presidency of Prof. Rücker, is a valuable instalment.

A. GRAY.

[1] From his book, "The New Attractive," &c., 1576.

PROGRESSIVE EDUCATION.[1]

THE publication of the second and third volumes of the series of reports prepared for the Education Department under the supervision of Mr. Michael E. Sadler, the Director of Special Inquiries, again calls the attention of educationists to the value of the work which is being performed by this latest addition to our national machinery for the guidance and coordination of educational effort. These reports, with their predecessors, closely resemble in their scope the publications of the U.S. Bureau of Education, and accentuate the fact that we have only lately commenced to do what has been for some time the established custom in the States. Just as the man of science acquaints himself with the work, failures and successes alike, of previous investigators in the field of his own activity, so to do the best possible work, the teacher, who is also an experimenter, should similarly become familiar with what has been accomplished by other teachers in different parts of the world. With these reports to hand, and others which will doubtless follow, there is no reason why the schoolmaster should not, in the future, start the education of his pupils armed with the experience of many successful veterans.

Though every one of the 1389 pages included in these volumes contains something of interest, this article is to be concerned more especially with what there is to be learnt concerning the present state of the teaching of science—whether in this country or abroad. Of the many plans of treatment which suggest themselves, perhaps, as reference to the volumes will be thereby facilitated, it will be best to take the papers containing important allusions to scientific instruction in the order in which they occur in the volumes themselves.

THE LONDON POLYTECHNIC INSTITUTES.

The London Polytechnic has but little resemblance to the institutions to which a similar name is applied in France, Switzerland, and Germany. Each Polytechnic Institute (of which there are at present eleven, with four branches) is an independent organisation which deliberately combines social intercourse, recreation, and instruction. As educational institutions these polytechnics constitute a new and distinct type; they range from the "Upper Standard" day school for boys and girls of thirteen, up to high University instruction and post-graduate research. All kinds and grades of work go on simultaneously. In one room boys of twelve are learning arithmetic, or girls of thirteen are being taught to sew; in another, classes of plumbers or bricklayers, compositors or tailors, are being practically trained in the processes of their respective crafts; close by is the smithy or the fitting shop, crowded with young engineering artisans; in other class-rooms are groups reading Dante or studying economics; and just at hand are well-equipped physical and chemical laboratories where

[1] "Special Reports on Educational Subjects." Vols. ii. and iii. (Issued by the Education Department, 1898.)

(as at Battersea this year) the teacher with a selected band of students is working out a Royal Society grant for research, or (as at Chelsea) graduates of more than one University are preparing their theses for the doctor's degree.

Six of the polytechnics now possess day schools and these have together about 1600 pupils, who are chiefly between twelve and fifteen years of age. Two of them are mixed schools for boys and girls, one has separate departments for the sexes, and three are confined to boys. They are in the main to be regarded as technical continuation schools. Seven day schools of a special type—the Domestic Economy School—are also attached to various polytechnics. In these the whole time of the pupil is spent in the study and practice of cookery, dressmaking, plain needlework, laundry work, and housewifery, with some elementary lessons in the chemistry of food and the physiology of hygiene. At Chelsea there is also a training school for servants.

At least 5000 workmen are at present attending the trade classes in connection with the London polytechnics. Not only is there a class in each branch of the building, engineering and metal, furniture, book and printing, and clothing trades, but also in such miscellaneous trades as baking, basket-making, and gas manufacture.

One polytechnic after another has found itself pushed into providing day instruction of various kinds, by the demand of students for whom evening classes are inaccessible or unnecessary. Thus the Battersea Polytechnic has regular day courses in mathematics and science, building and machine construction, wood-work and metal-work. The East London Technical College and the Regent Street Polytechnic have each a regular day engineering department, which turns out fully equipped engineers and electricians. At Regent Street a day architectural school has just been added. The Birkbeck Institution has a rapidly growing day department in natural science, including systematic courses in physics, chemistry, and biology. There are also day classes in Latin, Greek, and French, up to the standard of the B A. degree of London University. At Chelsea there is a fully developed Technical Day College for men and another for women.

More than one provincial city, proud of its "University College," counts fewer systematic day students than a single London Polytechnic.

In the science laboratories of the polytechnics every attempt is made to render the instruction both practical and scientific, and in addition to the crowds of elementary pupils there is generally a small body of enthusiastic advanced students who spend every hour they can spare in the laboratory, carrying on original research under the personal direction of the lecturers and demonstrators. This is sometimes systematised into what, in a German University, would be called a "Seminar." The research course at the Chelsea Polytechnic, under the direction of Mr. Herbert Tomlinson, F.R.S., may be cited.

The subject selected for the first research is "The effect of repeated heating on the magnetic permeability and electrical conductivity of iron and steel." The investigations are being accompanied by demonstrations and lectures on (1) the best methods of annealing iron and steel and the faults incidental thereto; (2) the determination of magnetic permeability both by ballistic and magnetometric methods; (3) the determination of the electrical conductivity of magnetic metals; (4) the critical temperature of iron and its alloys.

The method of conducting the research is, too, worthy of mention. To begin with, the principal selects some suitable subject, and fully explains to the class his reasons for such selection. He then gives a brief history of what has been previously done round and about the subject, and propounds a mode or modes of attacking the research, inviting criticisms from the class. The mode of attack having been decided upon, the class is expected not only to take part in the experiments but to help in making necessary apparatus. Should the results obtained be of sufficient importance, they are to be offered in the form of a paper to such societies as the Royal Society, the Physical Society, or the Institution of Electrical Engineers.

Enough has been culled from Mr. Sidney Webb's paper to show that the London Polytechnic is a pure addition to the educational system, neither competing with nor superseding existing institutions. There are, among all the 50,000 members and students of these new establishments, probably not a dozen who would have been found joining University or King's College had the polytechnics never come into existence. There is, Mr. Webb says, every indication that the whole 50,000 are a net gain, and represent the arrival of a poorer class of students than the University Colleges have as yet reached.

SCIENCE IN GIRLS' SCHOOLS.

Mrs. Bryant, headmistress of the North London Collegiate School for Girls, in a paper on "The curriculum of a girls' school," while admitting that the scientific interest arises almost as early in a child's mind as the literary and human interests, lays it down that the power to satisfy it develops much more slowly, so that the progress made is comparatively insignificant, even when the time spent is considerable and the methods sound. Nevertheless, it is recognised that the youngest children of school age are capable of elementary work in natural history. Such work at the beginning can hardly be called natural science, but it is of the nature of science if rightly carried out. An hour a week, increasing to two hours, may thus be profitably spent. At eleven or twelve children enter on physical science by very simple experimental investigations of the properties of matter. The ideal of the school curriculum in physical science is, it is suggested, a course of general elementary physics, starting with easy measurements and leading up to chemical problems, and eventually to chemistry. Mrs. Bryant thinks it is better for girls to carry on chemistry as a special study than to pursue any of the physical branches to the same extent. It has been found that backward pupils, and those who enter school late, get their best chance of science by the pursuit of botany and natural history as regular studies, with occasional courses on quasi-scientific subjects, like hygiene. All the pupils should, at some period of their course, deal with the application of science to life in some form, hence, short series of lessons, during one term, on hygiene and domestic economy, should be arranged. An interesting note by Miss Edith Aitkin, the science mistress in the school of which Mrs. Bryant is headmistress, gives in detail how the science teaching in the North London Collegiate School for Girls is carried out.

THE HEURISTIC METHOD OF TEACHING.

The art of making children discover things for themselves is the meaning Prof. Armstrong applies to the expression Heuristic method of teaching, and he contributes a very suggestive article on this theme to the second volume of the reports. After what is best described as an autobiographical account of his gradually developed belief in this system, Prof. Armstrong sketches historically the work of the British Association Committee and the Incorporated Association of Head Masters in formulating a scientific and logical introduction to physical and chemical studies. In 1888 the British Association Committee reported at the Bath meeting, that the replies received to a letter addressed to the headmasters of schools in which chemistry was taught—"have put them in possession of the actual facts connected with the teaching of chemistry in schools, and have made it clear that something should be done in the direction of promoting a more uniform and satisfactory treatment of the subject. The Committee think that some suggestions might now be made as to the method of teaching chemistry which should be followed in schools. If this can be done, it will certainly confer a great benefit on both teachers and examiners, and will be likely to lead to a more emphatic recognition of the merits of the science as an instrument of elementary education."

Two years later the same Committee recorded that—"it cannot be too strongly insisted that elementary physical science should be taught from the first as a branch of mental education, and not mainly as useful knowledge. It is a subject which, when taught with this object in view, is capable of developing mental qualities that are not aroused, and indeed are frequently deadened, by the exhaustive study of languages, history, and mathematics. In order that the study of physical science may effect this mental education, it is necessary that it should be employed to illustrate the scientific method in investigating nature, by means of observation, experiment and reasoning with the aid of hypothesis; the learners should be put into the attitude of discoverers, and should themselves be made to perform many of the experiments."

In 1895 a committee, appointed by the Incorporated Association of Headmasters, prepared a detailed syllabus of instruction in elementary physics and chemistry on the lines sketched out by the British Association Committee, and this syllabus was adopted by the headmasters in 1896. This detailed schedule has had a marked effect, both upon the personal teaching in

secondary schools, and also upon the character of the text-books which have been published from time to time for use in such schools.

Not only have many headmasters been won over to this newer and more intelligent mode of imparting scientific instruction, but the various examining bodies—the local examination authorities at Oxford and Cambridge, as well as the Senate of the London University—are all moving in the same direction. Examinations in elementary experimental science are becoming more and more practical. It is yearly becoming less common for examiners to be satisfied with verbal descriptions of scientific experiments, the candidates must be able to perform the experiments themselves and also draw the proper inferences from them.

In addition to other reasons why the heuristic method of teaching should be adopted, Prof. Armstrong claims for it that it conduces to the formation of moral and intellectual character and purpose. Children are encouraged to be properly inquisitive and inquiring; to observe correctly; to be neat and careful in all their work; to be economical; to attend patiently to details; to reason with judgment; to be handy, and to develop many other equally valuable characteristics.

For instruction in science to be carried out according to heuristic methods, ample room must be provided, but there need be no very special arrangements made. Prof. Armstrong enumerates the comparatively few necessaries in the way of fittings; they are: unfixed benches of the kitchen-table type, fitted with gas; one or two long sinks made of wood—elongated washing tubs; one or two benches fixed against the wall of the room, with cupboards (having a tray which will slide in and out) fixed in the space underneath; single draught-closet; considerable amount of wall space converted into blackboard; and free wall space, having upright battens affixed at regular intervals for attaching shelves or hooks. Apparatus should be provided to meet requirements as they arise, and "every effort should be made to utilise ordinary articles—medicine and pickle bottles, jam pots, saucepans, &c.—and to construct apparatus in the workroom; for this latter purpose a carpenter's bench and tools, vice and files, a small lathe, an anvil, and even a small forge should, whenever possible, form part of the equipment. Infinite injury is done at the present day, invaluable opportunities of imparting training are lost, by providing everything ready made." Centimetre-foot-rules, drawing-boards, T and set-squares, and balances *must*, however, be supplied. But the greatest of these is the balance. Says Prof. Armstrong, "altogether indispensable and essential as the primary weapon of heuristic instruction is the balance. There is no question that in the future the test of efficiency in a school will be the extent to which suitable balances are provided and used." Or, again, "the balance is to be regarded as an instrument of moral culture, to be treated with utmost care and reverence."

Appended to the article are the full courses of instruction in elementary science adopted by the Incorporated Association of Headmasters, and a course in botany framed by Prof. Marshall Ward, and included in the programme of the Joint Scholarships' Board Examinations.

PRUSSIAN SECONDARY EDUCATION FOR BOYS.

It is interesting to read, in the article entitled "Problems in Prussian Secondary Education for Boys," which Mr. Sadler contributes to vol. iii., of the part von Humboldt took in developing an effective plan of educational administration in Prussia. Humboldt was appointed Director of Public Education in 1792, and during the succeeding seventeen years was actively engaged in supplying the urgent need which was then felt for improved education. He insisted, among other things, that no one should be allowed to undertake higher education, even in private schools, who did not hold a certificate of competency awarded by a State authority. In 1808, he began to remodel Prussian secondary education; and the work of the succeeding ten years is carefully reviewed in Mr. Sadler's article. The eleventh section of this exhaustive inquiry is of especial importance at this time. It is concerned with the manner in which the growing need for more knowledge in the various branches of professional and commercial life has been met in Prussia. As Mr. Sadler says, "a really good secondary education, up to sixteen or nineteen as the case may be, does for a man what he can rarely do for himself in later years. It drills his intelligence, while the powers are still supple, and it presses into his mind, while the memory is still retentive and undisturbed by outside cares, a well-set foundation of necessary knowledge. But all over Germany these advantages have been made more accessible to the children of families of small means than is the case in England." It has been, moreover, a fixed principle in Prussia for some time, that the different types of secondary school should be kept distinct. "In the application of science to industry there has been an ever-increasing demand for young men of ability, well-trained in studies which equip them for business callings, instead of tending to estrange their sympathies from commercial life." The Education Department of Prussia now definitely accepts the idea of non-classical training as a part of a national system of higher education. The curricula and programmes of work for higher schools in Prussia, which follow the general article, should prove of great value in guiding English headmasters. That section which deals with natural science shows that in the gymnasium, or fully classical school, two hours a week are given in each form to the study of science. The teachers are instructed that "special importance is to be attached, not so much to the amount of what is learnt, as to the thoroughness with which it is studied." The endeavour must be "above all to guide the pupils to think and observe for themselves." In the Realgymnasium, or school with Latin only, and the Oberrealschule, or modern school, the amount of time given to science is increased in the higher forms to five hours a week in the Realgymnasium, and six hours a week in the Oberrealschule. It would take too much space to give a detailed account of the subjects, and the parts of them, studied in the different forms; but a reference to pp. 303-6 will give the necessary information.

HIGHER COMMERCIAL EDUCATION.

Mr. Sadler also contributes to vol. iii. an elaborate account of higher commercial education at Antwerp, Leipzig, Paris and Havre. The Institut Supérieur de Commerce at Antwerp aims at being a university for the future merchant, and at the special training of those to whom the consular service of the country will ultimately be entrusted. It is a public institution, under the inspection of the State, and its professors are civil servants. The Belgian government pays three-quarters of its annual cost, and the municipality of Antwerp the rest. Foreign students are admitted, and it is found that the associations thus formed are indirectly helpful to the furtherance of Belgian trade. The staff of the Institute consists of fourteen professors and two assistants, in addition to the director. To candidates who are successful in the final examination the Institute awards diplomas of merit, but these diplomas are not lightly given and are highly valued by business men. The diploma also qualifies a student to obtain one of the travelling scholarships awarded by the Government in order to encourage commercial inquiry. The scholarships are worth from 200*l.* to 250*l.* a year, and may be regarded as travelling studentships for commercial research.

One of the causes of the commercial advance of the German empire is the intellectual efficiency of the secondary schools, and of the higher Technical Institutes. "The secondary school is organised as the foundation, the higher Technical Institute as the crown." German non-classical secondary education prepares a boy to excel in commercial life, but the German secondary school authorities rigidly abstain on principle from any attempt at premature specialisation in commercial subjects. The first German Higher School of Commerce was last year established in Leipzig. There is to be a close correlation between this Higher School of Commerce and the University of Leipzig. The Director of the Leipzig School defines its objects "to be the raising of the position of the trading classes in social estimation, and their equipment with the higher level of expert knowledge which the conditions of modern industry require." Commercial opinion in Germany is not, however, unanimous in favour of the establishment of schools of commerce. Thus, in the Annual Report for 1897 of the Hamburg Chamber of Commerce the following remarks occur:—

"The science of business is a science which must be learned by practical experience. It cannot be picked up on the benches of a class-room. It must be acquired in practical life. A young man trained in a school of commerce will enter on practical life with his head full of all manner of preconceptions."

Mr. Sadler does not say much about the French Higher Schools of Commerce, but directs attention to the volume, "Commercial Instruction organised by the Paris Chamber of Commerce," which was prepared by that Chamber for the Chicago World's Fair of 1893. On the general question of commercial education, the following remarks are worthy of note. "All persons of experience heartily reprobate the

thrusting of so-called 'commercial subjects' into the curriculum of secondary day-schools. To cram up little boys of fifteen with odds and ends of commercial law and generalisations of commercial geography is to waste precious time which might have been devoted to subjects not only more elevating in themselves but more digestible by youthful minds."

CONCLUSION.

The foregoing sketch of some of the questions dealt with in the volumes under consideration does not exhaust the references to scientific instruction and higher education contained in them. Scattered through the reports are short allusions to other considerations which accentuate the importance of a fuller recognition of the claims of science as an effective instrument in education; and it is a happy augury that official publications of this kind should recognise so completely the good results which follow upon according science an important place in the curricula of secondary schools.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE annual meeting of the Association of Technical Institutions will take place on Thursday next, January 12, when an address will be delivered by the president, Lord Spencer.

THE canvassing committee under the Birmingham University scheme state that the total sum promised up to the date of the public meeting in the Council House in July last (95,658*l.*), had already been increased to about 115,000*l.* Mr. Frank McClean, F.R.S., has contributed the sum of 2000*l.* to the fund. The Lord Mayor of Birmingham announces a donation of 1000*l.* from Alderman T. S. Fallows, J.P. The committee will hold their next meeting on January 25, after which a second list of subscriptions will be published.

A WINTER meeting for teachers opened at the College of Preceptors on Monday, and will continue in session until the end of next week. The meeting was inaugurated by an address by Sir Joshua Fitch, and the programme also includes lectures on educational subjects, visits to educational institutions, a conference on the training of modern language teachers, a conference on science teaching, and a conversazione at the Clothworkers' Hall. The lectures will deal with the training of teachers, the practice of education (including the teaching of geography and of the "life sciences"), psychology applied to education, physiology applied to education, school hygiene, the training of the speaking voice, and other subjects. The conference on science teaching will be held at the South-West London Polytechnic, Manresa Road, Chelsea, next Thursday and Friday, and will consider the methods of teaching elementary experimental science (physics, chemistry and mechanics) in schools. In connection with the conference there will be an exhibition of apparatus used in schools for practical science teaching.

THE annual meeting of the Geographical Association will be held at the College of Preceptors on Wednesday, January 11, when an address will be given by the President, Mr. Douglas W. Freshfield. The Association was founded in 1893 with the object of improving the teaching of geography in secondary schools, and every one acquainted with what usually passes for geography instruction in such schools will acknowledge that there is much room for improvement. Rational methods of teaching experimental sciences are now being introduced into many schools, and geography ought to be brought into line with the new movement, for, when properly taught, it has a distinct educational value. All methods of science teaching which bring into play the pupil's intelligence, instead of merely loading the memory with names and isolated facts, should be encouraged; and as the work of the Association consists in spreading knowledge of improved methods of teaching geography, it deserves support. Intending members should communicate with Mr. J. S. Masterman, 55 Campden House Road, London, W.

A *Times* correspondent at Bombay reports that, on Saturday last, Lord Curzon received a deputation from the provisional committee of the proposed Imperial University, or Research Institute, to be established in India for the purpose of post-graduate instruction in the higher scientific and technical branches of learning. The petition referred to Mr. J. N. Tata's offer of property, representing a capital of 30 lakhs, and producing an income of a lakh and a quarter rupees, on trust to found an Imperial Institute, which it was thought would supply, with the help of the Government, the Native States, and the general public, this existing need. The present scheme was to found a separate institution open to graduates of all existing Universities, and fully equipped. The provisional committee submitted a draft Bill which had been prepared for the approval of the Government of India, with a scheme of studies and an estimate of the probable initial expenditure. The initial expenditure was put at 15 lakhs, and the annual charge at 3 lakhs. The committee asked for the support of the Government of India, as the proposed University was intended for all India, and, being Imperial in character, deserved special consideration from the supreme Government. The committee further requested the Government to sanction a grant in aid towards the annual expenditure, remarking that the Secretary of State for India would favour such a grant. If the Indian Government proposed to extend a grant in aid to higher studies, the native Princes would cordially co-operate. Some of them had contributed to the Jubilee Health Institute, ths object being bacteriological research, which fell within the confines of the committee's draft scheme. They suggested the amalgamation of the resources promised by the native Princes to the Jubilee Health Institute with Mr. Tata's donations, and pointed out that considerable funds had been raised in the Punjab and Haidarabad for research purposes. Lord Curzon, in reply to the deputation, said that he had carefully examined their representations, and, though he could, of course, give no final answer, he could, at all events, assure them that the object which they had in view had enlisted his warm sympathy.

PRESIDENT CRAFT'S annual report of the Massachusetts Institute of Technology shows that the past year was a remarkable one in the financial history of the Institute. More money was received through bequests and gifts than in any previous year. Under the will of the late Hon. Henry L. Pierce, seven hundred and fifty thousand dollars were paid to the Institute by his executors. This is the largest sum ever given to it by any one benefactor. In addition to this, the executors of the late Mrs. Julia B. H. James have paid over the very notable sum of one hundred and forty thousand and five hundred dollars, this being also one of the largest gifts ever made to the Institute. Mr. George A. Gardner has generously given twenty thousand dollars as a fund, the income from which is to be used in the payment of salaries. Ten thousand dollars came from the late John W. Carter, and fourteen hundred and eighty-two dollars has been added to the large sum previously received from the estate of the late Mrs. Susan E. Dorr for the Rogers Physical Laboratory. Besides these gifts to the Institute itself, a Travelling Fellowship in the Architectural Department has been established by the will of Mr. Willard B. Perkins. For this purpose the sum of six thousand dollars has been given, the accumulated income from which is to be used every fourth year. Forty thousand dollars came from the estate of the late Mrs. Ann White Dickinson, the whole sum for scholarship purposes. A friend has given five hundred dollars to meet a special want, and two hundred dollars has come from Mrs. William B. Rogers, to be used for periodicals. It is expected that the Institute will receive four hundred thousand dollars from the estate of the late Mr. Edward Austin. This amount appears to be intended for scholarships and other similar uses, and will be highly appreciated; but the great desideratum for the immediate future is accessions to the unrestricted funds of the Institute.

The actual expense of instructing the students at the Massachusetts Institute is on the average three hundred and thirty dollars per year, while only two hundred dollars is paid as tuition fees. The balance, one hundred and thirty dollars, including interest on permanent investments, land, buildings, machinery, &c., has to be met from the past and present government and private benefactions. All through the reports from the different departments of the Institute notices are given of the introduction of advanced studies in consequence of advanced entrance requirements, and the school is making continued progress towards a higher standard for its degree. Another noteworthy feature is the progress towards a greater subdivision of students into small sections in laboratories, and the constantly increasing value placed upon laboratory work. During the past four years this movement has led to the appointment of eighteen new instructors, while the total number of students has remained about the

same. The number of students last year was 1171. If any one figure can be taken as a measure of the efficiency of a well-conducted school, it is the ratio of the total number of students to the number of instructors in actual service. In the case of the Institute of Technology, without counting lecturers, there is one instructor to every eight or nine students—one of the very highest ratios in the United States.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 8, 1898.—"Mathematical Contributions to the Theory of Evolution. VI. Reproductive or Genetic Selection. Part I. Theoretical." By Karl Pearson. "Part II. On the Inheritance of Fertility in Man." By Karl Pearson and Alice Lee. "Part III. On the Inheritance of Fecundity in Thoroughbred Race-horses." By Karl Pearson, with the assistance of Leslie Bramley-Moore.

The object of this memoir is twofold: first, to develop the theory of reproductive or genetic selection [1] on the assumption that fertility and fecundity may be heritable characters; and, secondly, to demonstrate from two concrete examples that fertility and fecundity actually are inherited.

The problem of whether fertility is or is not inherited, is one of very far reaching consequences. It stands on an entirely different footing to the question of inheritance of other characters. That any other organ or character is inherited, provided that inheritance is not stronger for one value of the organ or character than another, is perfectly consistent with the organic stability of a community of individuals. That fertility should be inherited is not consistent with the stability of such a community, unless there be a differential death-rate, more intense for the offspring of the more fertile, *i.e.* unless natural selection or other factor of evolution holds reproductive selection in check. The inheritance of fertility and the correlation of fertility with other characters are principles momentous in their results for our conceptions of evolution; they mark a continual tendency in a race to progress in a definite direction, unless equilibrium be maintained by any other equipollent factors, exhibited in the form of a differential death-rate on the most fertile. Such a differential death-rate probably exists in wild life, at any rate until the environment changes and the equilibrium between natural and reproductive selection is upset. How far it exists in civilised communities of mankind is another and more difficult problem, which I have partially dealt with elsewhere. At any rate it becomes necessary for the biologist either to affirm or deny the two principles stated above. If he affirms them, then he must look upon all races as tending to progress in definite directions—not necessarily one, but possibly several different directions, according to the characters with which fertility may be correlated—the moment natural selection is suspended; the organism carries in itself, in virtue of the laws of inheritance and the correlation of its characters, a tendency to progressive change. If, on the other hand, the biologist denies these principles, then he must be prepared to meet the weight of evidence in favour of the inheritance of fertility and fecundity contained in Parts II. and III. of the present memoir.

The theory discussed in Part I. opens with the proof that if fertility be a function of any physical characters which are themselves inherited according to the law of ancestral heredity, then it must itself be inherited according to that law. As fertility would certainly appear to be associated with physique, we have thus an *à priori* argument in favour of its inheritance.

Further points dealt with are the influence of "record-making" on apparent fertility and fecundity. The fertility of mothers is always found to be more and their variability less than the fertility and variability of daughters. Accordingly from the apparent fertility and variability of the record the actual values in each generation must be deduced.

Methods are developed for finding correlation coefficients from the means of "arrays." These methods are of considerable importance, for they enable us to ascertain the correlation between a latent character in one sex and a patent character in another, or between characters latent in two individuals. Thus, it is shown that the correlation between the brood-mare's fecundity latent in two related stallions can be deduced from the correlation between the mean fecundities of their two arrays of daughters. In this way a numerical estimate can be formed of the inheritance of latent characters.

[1] I have retained the term "reproductive" selection here, although objection has been raised to it, because it has been used in the earlier memoirs of this series. Mr. Galton has kindly provided me with "genetic" and "proliferal" selection. The term is used to describe selection of predominant types owing to the different grades of reproductivity being inherited, and without the influence of a differential death-rate.

The effect of a mixture of correlated and uncorrelated material on correlation and variation is next investigated, and it is shown that the former is more seriously affected than the latter. Incidently the problem of the mixture of heterogeneous materials uncorrelated in themselves is investigated, and it is shown that a correlation will result in the mixture. This *spurious* correlation is of some importance for the question of mixtures of classes in fertility problems, but it is also significant of the general danger of heterogeneity in bio-statistical investigations, and further indicative of the possibility of creating correlation between two characters by breeding between small heterogeneous groups in which this correlation is zero.

Part II. of the memoir deals with the inheritance of fertility in man. It is first shown by large numbers that fertility is undoubtedly inherited from mother to daughter, but that if we include all types of marriages the inheritance is largely screened by other factors. An attempt is made to remove one by one these factors, and the more stringently this is done the more nearly the regression of daughter on mother moves up towards he value required by the law of ancestral heredity.

The inheritance of fertility from father to son is then considered; this is really rather an inheritance of sterility or tendency to sterility, for the full fecundity of a man is not usually exhibited in monogamic union. It is rather a problem of whether his fecundity lasts as long as his wife's. We find definite inheritance from father to son of this sterile tendency, although for the reason just given it falls below that indicated by the law of ancestral heredity.

Lastly, the inheritance of fertility in the woman through the male line is dealt with, and it is shown that a woman's fertility is as highly correlated with that of her paternal as with that of her maternal grandmother. In other words the latent character, fertility in the woman, is transmitted through the male line, and with an intensity which approximates to that required by the law of ancestral heredity.

Part III. of the memoir contains the results of a somewhat laborious investigation into the fecundity of brood-mares, which has been a number of years in progress.

(1) Fecundity in the brood-mare is inherited from dam to mare.

(2) It is also inherited from grand-dam to mare through the dam.

In both these cases the intensity is much less than would be indicated by the law of ancestral heredity, but the divergence is not such that it could not be accounted for by a percentage of fictitious values such as the peculiar conditions of horse-breeding warrant us in considering probable.

(3) The latent quality, fecundity in the brood-mare, is inherited through the sire; this is shown not only by the correlation between half-sisters, but by actual determination of the correlation between the latent character in the sire and the patent character in the daughter.

(4) The latent quality, fecundity in the brood-mare, is inherited by the stallion from his sire. This is shown not only by the fecundity correlation between a sire's daughters and his half-sisters, but also by a direct determination of the correlation between the latent quality in the stallion and in his sire.

In both these cases of latent qualities the law of inheritance approaches much more closely to that required by the Galtonian rule.

Parts II. and III. accordingly force us to the conclusion that fertility is inherited in man and fecundity in the horse, and therefore probably that both these characters are inherited in all types of life. It would indeed be difficult to explain by evolution the great variety of values these characters take in allied species, if this were not true. That they are inherited according to the Galtonian rule seems to us very probable, but not demonstrated to certainty. It is a reasonable hypothesis until more data are forthcoming.

PARIS.

Academy of Sciences, December 26, 1898.—M. Wolf in the chair.—Some peculiarities of the elasticity of muscle explained by comparison of the case of muscular substance in action with that of inert materials, by M. A. Chauveau. An experimental

comparison of the elasticity of vulcanised india-rubber with that of muscle in a state of physiological work. The results are expressed graphically in a series of six diagrams.—Histology of the skin: the epidermal fat of birds, by M. L. Ranvier. A study of the cause of the difference between the epidermal wax of mammals and the fatty material obtained from the feet of birds. A tangential section from the scaly portion of a chicken's foot, stained with osmic acid, showed that the epidermal cells are charged with liquid oily drops.—On a photograph of the nebula of Baleine obtained at the Observatory of Toulouse, by MM. Bailland and Bourget. The spiral in the nucleus, first described by Lord Rosse, is very clearly marked in the photograph.—Observations and elements of the new Chase comet, by M. G. Fayet.—On differential systems the integration of which can be reduced to that of total differential equations, by M. Riquier.—On differential equations of the first order, by M. Armand Cahen.—On linear total differential equations, by M. Alf. Guldberg.—On the velocity of sound in dry air at 0° C., by M. A. Leduc. A reply to the criticisms of M. Violle.—Influence of pressure on the initial polarisation capacity, by M. A. Chassy. Polarisation cells containing electrodes of various metals, as well as platinum black, were submitted to pressures ranging from 1 to 2000 atmospheres, without any appreciable change in the value of the polarisation capacity being noticeable. It is hence concluded that the phenomenon of initial polarisation does not correspond to an electrolytic decomposition into gaseous elements.—Radioconductors, with gold and platinum filings, by M. Édouard Branly. The use of gold powder for the preparation of tubes used in the detection of Hertzian waves has been negatived by Prof. Oliver Lodge; but the author finds, on the contrary, that although the successful use of tubes of this metal requires more delicate handling than other powders, this objection is more than counterbalanced by the extreme sensitiveness of the apparatus.—On a solution of the problem of multicommunication in telegraphy by the use of electric oscillations, by M. Albert Turpain. Having given several stations, A, B, C, . . . L, N, distributed along a single wire A N, the arrangement described permits of the simultaneous telegraphic or telephonic transmission of message between any pair of stations.—Remarks on the simple kathode rays, by M. H. Deslandres. A discussion of the views of M. Goldstein, and of his claims to priority on this subject.—On a new strongly radio-active substance contained in pitchblende, by M. and Mme. P. Curie and M. G. Bémon (see p. 232).—On the spectrum of a new radio-active substance, by M. Eug. Demarcay.—Researches on the separation and estimation of the halogen elements in their combinations with silver, by M. H. Baubigny. The mixed halogen silver salts are treated in the moist state with sulphuric acid and potassium bichromate at 90°–95° C. Under these conditions the iodide is quantitatively converted into iodate, and the chlorine and bromine set free. The test analyses are very satisfactory.—Action of oxidising agents upon some fatty and aromatic amines, by MM. Œchsner de Coninck and A. Combe. In presence of energetic oxidising agents the fatty amines are gradually decomposed, giving CO_2 and N_2. Under the same conditions the aromatic amines give colouring matters which are slowly decomposed with evolution of CO_2.—Thermal study of normal propylmalonic acid. Heat of formation of the solid potassium salt, by M. G. Massol.—On the oxidation products of oxygluconic acid, by M. Léon Boutroux. Oxidation with nitric acid gave racemic, trioxyglutaric, glyoxylic, and dioxybutyric acids.—The anti-virulent power of the serum of man and animals immunised against vaccinal or variolic infection, by MM. Béclère, Chambon, Ménard, and Jousset.—On an example of *Dasypeltis scabra*, by M. Léon Vaillant.—Fall of fossilised crustacean ostracods observed at Oullins, near Lyons, September 24, by M. Lortet.—Contribution to the study of the morphology of the Craspedomonadeæ, by M. J. Kunstler.—On the presence of *Rinus sylvestris* in the quaternian gravels near Troyes, by M. P. Fliche. The remains found consist of the wood, roots, branches, bark, and cones, the latter being especially well preserved. The fossils do not occur in the base of the gravel, but are very abundant in the middle region.—On the discovery of graptoliths in the conglomerates of the vosgian grit in the neighbourhood of Raon-l'Étape, by M. Bleicher.—Appearance of the bear in the Miocene period, by M. Claude Gaillard.—On the folding of strata near Belledone, by M. P. Lory.—On the foldings of the Cretaceous strata of the Aquitaine basin, by M. Ph. Glangeaud.—On the origin of the iron ore of the Neocomian at Bray, by the superficial alteration of iron carbonate, and on the importance and continuity in depth of the carbonated ore, by M. W. de Mercey.—Observations by M. de Lapparent on the preceding paper.—On the existence near Corinth of lherzolites identical with those found in the Pyrenees, by M. A. Lacroix.—On a new law relating to the grouping of crystals, by M. Fr. Wallerant.—New researches on a means of preventing oak wood from being worm-eaten, by M. Émile Mer.

DIARY OF SOCIETIES.

TUESDAY, JANUARY 10.

INSTITUTION OF CIVIL ENGINEERS, at 8.—High-Speed Engines: John Handsley Dales.

ANTHROPOLOGICAL INSTITUTE, at 8.30.—On Micronesian Weapons, Dress, Implements, &c.: F. W. Christian.

WEDNESDAY, JANUARY 11.

SOCIETY OF ARTS, at 7.—Juvenile Lecture: Some Ways in which Animals Breathe: Prof. F. Jeffrey Bell.

THURSDAY, JANUARY 12.

MATHEMATICAL SOCIETY, at 8.—On a Determinant each of whose Elements is the prduct of k Factors: Prof. W. H. Metzler.—Properties of Hyper-space, in relation to Systems of Forces, the Kinematics of Rigid Bodies, and Clifford's Parallels: A. N. Whitehead.—A Simple Method of Factorising Large Composite Numbers of any unknown form: D. Biddle.—Zeroes of the Bessel Functions (second paper): H. M. Macdonald.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.

BOOKS and SERIALS RECEIVED.

BOOKS.—Catalogue of the Lepidoptera Phalænæ in the British Museum: Sir G. F. Hampson (London).—Plates to ditto.—The Brain Machine: Dr. A. Wilson (Churchill).—Les Terres Rares: P. Truchot (Paris, Carré).—Advanced Inorganic Chemistry: Dr. G. H. Bailey (Clive).—The Principles of Stratigraphical Geology: J. E. Marr (Cambridge University Press).—Octonions: Prof. A. McAulay (Cambridge University Press).—Scientific Papers of Prof. P. G. Tait, Vol. i. (Cambridge University Press).—Spherical Trigonometry: W. W. Lane (Macmillan).—Introduction to the Theory of Analytic Functions; Profs. Harkness and Morley (Macmillan).

SERIALS.—Longman's Magazine, January (Longmans).—Agricultural Gazette of New South Wales, October (Sydney).—National Review, January (Arnold).—Bulletin of the American Mathematical Society, December (N.Y., Macmillan).—Physical Review, September–October (Macmillan).—Humanitarian, January (Duckworth).—Contemporary Review, January (Isbister).—Journal of the Royal Horticultural Society, January (117 Victoria Street).—Journal of the Royal Microscopical Society, December (Williams).—Journal of Experimental Medicine, Vol. 3, No. 6 Appleton).—American Naturalist, December (Ginn).—Brain, Part 83 Macmillan).

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THURSDAY, JANUARY 12, 1899.

THE DEVELOPMENT OF RESEARCH.

The Discharge of Electricity through Gases. By Prof. J. J. Thomson, D.Sc., F.R.S. Pp. 203. (Westminster: Archibald Constable and Co., 1898.)

THIS work is an expansion of a series of lectures delivered at the University of Princeton, New Jersey, U.S.A., in October 1896. This practice of the American universities of inviting distinguished authorities to deliver courses of lectures is an admirable one. Lord Kelvin's Baltimore lectures were delivered under similar auspices, and it is to be hoped that the promised publication of these most interesting lectures will soon take place. By thus bringing our greatest living authorities into personal relations with the staff and students of a university an energetic stimulus is given to their studies and investigations. It might be said that the local staff and students can, if they wish, read the works of any authorities they desire to study. This is no doubt true, and it makes it now possible for each university by producing new knowledge to teach all the world, and not only those who come to reside in its vicinity. Thus the real students of each university are now spread all over the world and not confined to its precincts, as they were before the reproduction and distribution of thought was as easy a matter as it is to-day. And this is a great and important duty for universities, this producing and teaching new knowledge to mankind, but it is not their only duty. They should produce investigators and discoverers as well as investigations and discoveries. Under existing conditions, investigations and discoveries in the borderlands of science cannot reasonably be expected to lead to immediately useful results to mankind. They may be most useful to our grandchildren, but one cannot reasonably expect the ordinary motives of self-interest, to which the greater part of the good work of the world is due, to produce great work which may be of use to other people's grandchildren. For the production of such work, society, which has the greatest interest in the matter, must depend upon other motives for the production of great discoveries in pure science. The motives and abilities that must here be depended on do not exist in at all the same proportion of mankind as self-interest and that general ability to carry out rules which is sufficient for so much of the world's work. Enthusiastic devotion to the investigation and discovery of what is true, and the intellectual acuteness required for its successful pursuit, are not of common occurrence, and in a great many cases without special encouragement and training will, even in those who are capable of having these capacities highly developed, be overpowered by the distractions of other motives claiming attention to other fields of work. It is an important duty for universities to seek out those in whom it is possible to develop these motives and abilities; to encourage them to cultivate these abilities, and to strengthen in every way the hold of these motives on them. Almost the only way in which such motives can be strengthened is by the sympathetic encouragement of those who are already enthusiastic investigators and discoverers. That this is a really successful way of producing the character desired is fully proved by the existence of *disciples* in every branch of human endeavour that involves enthusiastic devotion of life. How is it that so many of the passing generation of chemical discoverers have been workers in the laboratories of Liebig, for example? The proportion of chemists who studied with Liebig to the whole body of those who have studied chemistry is very small indeed, but the proportion of leaders of chemical discovery who have studied under Liebig to the whole number of leaders of chemical discovery is quite large. Why? Because Liebig's example was catching, his personality was inspiring, his enthusiasm begot enthusiasm in his pupils; they became more than pupils, they became disciples.

It is in pursuit of this end that the American universities desire to bring their staff and students into personal relations with the leaders of thought. Though there is every prospect that they soon will be, they are not yet rich enough to control the market of English-speaking thought production by making permanent appointments, but they can and do provide for a temporary residence of a leader of thought amongst them. They do what they can to bring themselves into personal relations with those who are likely to stir their enthusiasm for research into useless truth, and thus strengthen the motive which distinguishes investigators and discoverers of the kind universities should produce.

These lectures are eminently inspiring. They are full of the seed from which discovery springs, of those sign-posts on the borderlands of the known which point out directions in which further knowledge is required. They call attention to important investigations now in progress, to the results so far achieved and to the results, too, that are still only probable and require further investigation. We do not generally get this in a book. We may find it in published papers, but we should find it in lectures delivered for the purpose of directing and encouraging the researches of others. So much of this is of only ephemeral value, though, like our dinners, of enormous importance for each day's work, that it is not usually published in book form nor often in scientific papers, notwithstanding such an illustrious example as Faraday. Working hypotheses, suggestions for work, all these may turn out wrong, or useless for further advance, but may be a very necessary part of the means of advancing. We eat much that we cannot assimilate in our dinners: it is a necessary concomitant of our food, and a reasonable quantity of it is possibly necessary for the satisfactory working of our organisms. Be that as it may, the question of real importance in working hypotheses is not so much, were they true, as, did they lead to advance towards truth. Very few, indeed, who have not suggested many wrong working hypotheses have advanced truth at all, and still fewer have had enthusiastic disciples.

In these lectures we have studies of the possible causes of atmospheric electricity depending on electro-superficial double layers, a subject deserving most careful investigation; studies of a remarkable penetration of kathode ray actions through such thick conductors that Lenard rays seem out of the question, and that raises the important question of the continuity or otherwise of the

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